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THE PRINCIPLES OF SCIENCE:

A TREATISE ON LOGIC

AND

SCIENTIFIC METHOD.

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VOL. II.

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London

MACMILLAN AND CO.

1874

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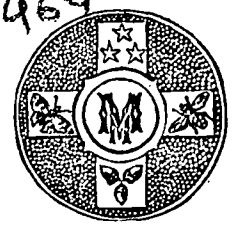

OXFORD:
E. PICKARD HALL, AND J. H. STACY,
PRINTERS TO THE UNIVERSITY.

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BOOK IV.

INDUCTIVE INVESTIGATION.

CHAPTER XVIII.

OBSERVATION.

ALL knowledge proceeds originally from experience. Using the name in a wide sense we may say that experience comprehends all that we *feel*, externally or internally—the aggregate of the impressions which we receive through the various apertures of perception—the aggregate consequently of what is in the mind, except so far as some portions of knowledge may be the reasoned equivalents of other portions. As the word experience implies[a], we *go through* much in life, and the impressions gathered intentionally or unintentionally afford the materials from which the active powers of the mind evolve science.

No small part of the experience actually employed in science is acquired without any distinct purpose. We cannot use the eyes without gathering some facts which may prove useful. Every great branch of science has generally taken its first rise from an accidental observation. Erasmus Bartholinus thus first discovered double refraction in Iceland spar; Galvani noticed the twitching of a frog's leg; Oken was struck by the form of a

[a] Max Müller's 'Lectures on Language,' vol. ii. p. 73.

vertebra; Malus unintentionally examined with a double refracting substance light reflected from distant windows; and Sir John Herschel's attention was drawn to the peculiar appearance of a solution of quinine sulphate. In earlier times there must have been some one who first noticed the strange behaviour of a loadstone, or the unaccountable motions produced by amber. As a general rule we shall not know in what direction to look for a great body of phenomena widely different from those familiar to us. Chance then must give us the starting point; but one accidental observation well used may lead us to make thousands of observations in an intentional and organized manner, and thus a science may be gradually worked out from the smallest opening.

Distinction of Observation and Experiment.

It is usual to say that the two modes of experience are Observation and Experiment. When we merely note and record the phenomena which occur around us in the ordinary course of nature we are said *to observe.* When we change the course of nature by the intervention of our will and muscular powers, and thus produce unusual combinations and conditions of phenomena, we are said *to experiment.* Sir John Herschel has justly remarked[b] that we might properly call these two modes of experience *passive and active observation.* In both cases we must certainly employ our senses to observe, and an experiment differs from a mere observation in the fact that we more or less influence the character of the events which we observe. Experiment is thus observation *plus* alteration of conditions.

It may readily be seen that we pass upwards by insensible gradations from pure observation to determinate

[b] 'Preliminary Discourse on the Study of Natural Philosophy,' p. 77.

experiment. When the earliest astronomers simply noticed the ordinary motions of the sun, moon, and planets upon the face of the starry heavens, they were pure observers. But astronomers now select precise times and places for important observations of stellar parallax, or the transits of planets. They make the earth's orbit the basis of a well arranged natural experiment, as it were, and take well considered advantage of motions which they cannot control. Meteorology might seem to be a science of pure observation, because we cannot possibly govern the changes of weather which we record. Nevertheless we may ascend mountains, or rise in balloons, like Gay-Lussac and Glaisher, and may thus so vary the points of observation as to render our procedure experimental. We are wholly unable either to produce or prevent earth currents of electricity, but when we construct long lines of telegraphic wires, we gather such strong currents during periods of disturbance as to render them capable of easy observation.

The most well arranged and assiduous systems of observation, however, would fail to give us a large part of the facts which we now possess. Many of the processes which are continually going on in nature may be so slow and gentle in operation that they would for ever escape our powers of observation. Lavoisier remarked that the decomposition of water must have been constantly proceeding in nature, although its possibility was unknown till his time[c]. No substance is wholly destitute of magnetic or diamagnetic powers; but it required all the experimental skill of Faraday to prove that iron, and a few other metals had no monopoly of these powers. Passive and accidental observation long ago impressed upon men's minds the phenomena of lightning, and the attractive properties of amber. Experiment only could have shown that phenomena so diverse in magnitude and

[c] Lavoisier's 'Elements of Chemistry,' transl. by Kerr, 3rd ed. p. 148.

character were manifestations of the one same agent. To observe with accuracy and convenience we must have agents under our control, so as to raise or lower their intensity, to stop or set them in action at will. Just as Smeaton found it requisite to create an artificial and governable supply of wind for his investigation of windmills, so we must have constant or governable supplies of light, heat, electricity, muscular force, or whatever other agents we are examining.

It is hardly needful to point out too that on the earth's surface we live under nearly uniform conditions of gravity, temperature, and atmospheric pressure, so that if we are to extend our inferences to other parts of the universe where conditions may be widely different, we must be prepared to imitate those conditions on a small scale here. We must have intensely high and low temperatures; we must vary the density of gases from approximate vacuum upwards; we must subject liquids and solids to pressures or strains of almost unlimited amount.

Mental Conditions of Correct Observation.

Every observation must in a certain sense be true, for the observing and recording of an event is in itself an event. But before we proceed to deal with the supposed meaning of the record, and draw inferences concerning the course of nature, we must take care to ascertain that the position and feelings of the observer are not to a great extent the phenomena recorded. The mind of man, as Francis Bacon said, is like an uneven mirror, and does not reflect the events of nature without distortion. We need not take notice of intentionally false observations, nor of mistakes arising from defective memory, deficient light, and so forth. Even where the utmost intentional fidelity and care are used in observing and recording,

tendencies to error exist, and fallacious opinions arise in consequence.

It is exceedingly rare to find persons who can with perfect fairness estimate and register facts for and against their own peculiar views and theories. Among uncultivated observers the tendency to remark favourable and forget unfavourable events is so great, that no reliance can be placed upon their supposed observations. Thus arises the enduring fallacy that the changes of the weather coincide in some way or other with the changes of the moon, although exact and impartial registers give no countenance to the fact. The whole race of prophets and quacks live upon the overwhelming effect of one success, compared with hundreds of failures which are unmentioned and forgotten. As Bacon says, 'Men mark when they hit, and never mark when they miss.' We should do well to bear in mind the ancient story, quoted by Bacon, of one who in Pagan times was shown a temple with a picture of all the persons who had been saved from shipwreck, after paying their vows. When asked whether he did not now acknowledge the power of the gods, 'Aye,' he answered; 'but where are they painted that were drowned after their vows?'

If indeed we could estimate the amount of *bias* existing in any particular observations, it might be treated like one of the forces of the problem, and the true course of external nature might still be rendered apparent. But the feelings of an observer are usually too indeterminate, so that whenever there is reason to suspect any considerable amount of bias, rejection is the only safe course. As regards facts casually registered in past times, the capacities and impartiality of the observer are so little known that we should spare no pains to replace these statements by a new appeal to nature. An indiscriminate medley of truth and absurdity, such as Francis Bacon has collected in his

Natural History, is wholly unsuited to the purposes of science. But of course when records relate to past events like eclipses, conjunctions, meteoric phenomena, earthquakes, volcanic eruptions, changes of sea margins, the existence of now extinct animals, the migrations of tribes, floods, &c., we must depend upon traditions or records, however unsatisfactory, and must endeavour to verify the statements by the comparison of independent records.

When extensive series of observations have to be made, as in astronomical, meteorological, or magnetical observatories, trigonometrical surveys, and extensive chemical or physical researches, it is an advantage that the numerical estimations and records should be executed by assistants who are not interested in, and are perhaps unaware of, the expected results. The record is thus rendered perfectly impartial. It may even be desirable that those who perform the purely routine work of measurement and computation should be unacquainted with the principles of the subject. The great table of logarithms of the French Revolutionary Government was worked out by a staff of sixty or eighty computers, most of whom were acquainted only with the rules of arithmetic, and worked under the direction of skilled mathematicians; yet their calculations were usually found more correct than those of persons more deeply versed in mathematics[d]. In the Indian Ordnance Survey the actual measurers have been selected so that they shall not have sufficient skill to falsify their results without detection.

Both passive observation and experimentation must, however, be generally conducted by persons who know for what they are to look. It is only when excited and guided by the hope of verifying a theory that the observer will notice many of the most important points; and, where the work is not of a routine character, no

[d] Babbage, 'Economy of Manufactures,' p. 194.

assistants can supersede the mind-directed observations of the philosopher. Thus the successful investigator must combine diverse qualities; he must have clear notions of the result he expects, and confidence in the truth of his theories, and yet he must have that candour and flexibility of mind, which enable him to accept unfavourable results and abandon mistaken views.

Instrumental and Sensual Conditions of Correct Observation.

In every observation one or more of the senses must be employed, and we should ever bear in mind that the extent of our knowledge may be limited by the power of the sense concerned. What we learn of the world only forms the lower limit of what is to be learned, and, for all that we can tell, the processes of nature may indefinitely surpass in variety and complexity those which are capable of coming within our means of observation. In some cases inference from observed phenomena may make us indirectly aware of what cannot be directly felt, but we can never be sure that we thus acquire any appreciable fraction of the knowledge that might be acquired.

It is a strange reflection that space may be filled with dark wandering stars, whose existence could not have yet become in any way known to us. The planets have already cooled so far as to be no longer luminous, and it may well be that other stellar bodies of various size have fallen into the same condition. From the consideration, indeed, of variable and extinguished stars, Laplace inferred that there probably exist opaque bodies as great and perhaps as numerous as those we see[e]. Some of these dark stars might ultimately become known to us, either by reflecting light, or more probably by their gravitating

[e] 'System of the World,' translated by Harte, vol. ii. p. 335.

effects upon luminous stars. Thus if one member of a double star were dark, we could readily detect its existence, and even estimate its size, position, and motions, by observing those of its visible companion. It was a favourite notion of Huyghens that there may exist stars and vast universes so distant that their light has never yet had time to reach our eyes; and we must also bear in mind that light may possibly suffer slow extinction in space, so that there is more than one way in which an absolute limit to the powers of telescopic discovery may exist.

There are natural limits again to the power of our senses in detecting undulations of various kinds. It is commonly said that vibrations of less than sixteen strokes or more than 38,000 strokes per second are not audible as sound; and as some ears actually do hear sounds of much higher pitch, even two octaves higher than what other ears can detect, it is exceedingly probable that there are incessant vibrations which we cannot call sound because they are never heard. Insects may possibly communicate by such acute sounds, constituting a language inaudible and inscrutable to us; and the remarkable agreement apparent among bodies of ants or bees might thus perhaps be explained. Nay, as Fontenelle long ago suggested in his scientific romance, there may exist unlimited numbers of senses or modes of perception which we can never feel, though Darwin's theory would render it probable that any useful means of knowledge in an ancestor would be developed and improved in the descendants. We might doubtless have been endowed with a sense capable of feeling electric phenomena with acuteness, so that the positive or negative state of charge of a body could be at once estimated. The absence of such a sense is probably due to its comparative uselessness.

Heat undulations are subject to the same considerations.

It is now apparent that what we call light is the affection of the eye by certain vibrations, the less rapid of which are invisible and constitute the dark rays of radiant heat, in detecting which we must substitute the thermometer or the thermopile for the eye. At the other end of the spectrum, again, the ultra-violet rays are invisible, and only indirectly brought to our knowledge in the phenomena of fluorescence or photo-chemical action. There is no reason to believe that at either end of the spectrum an absolute limit has yet been reached.

Just as our knowledge of the stellar universe is limited by the power of the telescope and other conditions, so our knowledge of the minute world has its limit in the powers and optical conditions of the microscope. There was a time when it would have been a reasonable induction that vegetables were motionless, and animals alone endowed with power of locomotion. We are astonished to discover by the microscope that minute plants are if anything more active than minute animals. We even find that mineral substances seem to lose their inactive character and dance about with incessant motion when reduced to sufficiently minute particles, at least when suspended in a non-conducting medium[f]. Microscopists will meet a natural limit to their means of observation when the minuteness of the objects examined becomes comparable to the length of light undulations, and the extreme difficulty already encountered in determining the forms of minute marks on Diatoms appears to be due to this cause.

Of the errors likely to arise in estimating quantities by the senses I have already spoken (vol. i. p. 320), but there are some cases in which we actually see things different from what they are. A jet of water often appears to be a continuous thread, when it is really a wonderfully or-

[f] Jevons, 'Proceedings of the Literary and Philosophical Society of Manchester,' 25th January, 1870, vol. ix. p. 78.

ganized succession of small and large drops, oscillating in form. The drops fall so rapidly that their impressions upon the eye run into each other, and in order to see the separate drops we require some device for giving an instantaneous view, such as illumination by the electric spark, or the use of the revolving disc called the phenakistiscope.

One insuperable limit to our powers of observation arises from the impossibility of following and identifying the ultimate atoms of matter. One atom of oxygen is probably undistinguishable from another atom; only by keeping a certain volume of oxygen safely enclosed in a bottle can we assure ourselves of its identity; allow it to mix with other oxygen, and we have lost all power of identification. Accordingly we seem to have no means of directly proving that every gas is in a constant state of diffusion of every part into every part. We can only infer this to be the condition from observing the behaviour of distinct gases which we can distinguish in their course, and by reasoning on the grounds of molecular theory [g].

External Conditions of Correct Observation.

Before we proceed to draw inferences from any series of recorded facts, we must take great care to ascertain perfectly, if possible, the external conditions under which the facts are brought to our notice. Not only may the observing mind be prejudiced and the senses defective, but there may be circumstances which cause one kind of event to come more frequently to our notice than another. The comparative numbers of events or objects of different kinds existing may in any degree differ from the numbers which we are able to record. This difference must if possible be taken into account before we make any inferences.

[g] Maxwell, 'Theory of Heat,' p. 301.

There long appeared to be a strong presumption that all comets moved in elliptic orbits, because no comet had been proved to move in any other kind of path. The theory of gravitation admitted of the existence of comets moving in hyperbolic orbits, and the question arose whether they were really non-existant or were only beyond the bounds of easy observation. From reasonable suppositions Laplace calculated that the probability was at least 6000 to 1 against a comet which comes within the planetary system sufficiently to be visible at the earth's surface, presenting an orbit which could be discriminated from a very elongated ellipse or parabola, in the part of its orbit within the reach of our telescopes[h]. In short, the chances are very much in favour of our seeing elliptic rather than hyperbolic comets. Laplace's views have been confirmed by the discovery of six hyperbolic comets, which appeared in the years 1729, 1771, 1774, 1818, 1840, and 1843[i], and, as only about 800 comets altogether have been recorded, the proportion of hyperbolic ones is quite as large as should be expected. Some remarkable speculations have recently been published by Mr. A. S. Davies, as to the probable character of the orbits of comets, which, after moving freely through space, become attached to this planetary system[k].

When we attempt to estimate the numbers of objects which may have existed, we must make large allowances for the limited sphere of our observations. Thus probably not more than 4000 or 5000 comets have been seen in historical times, but making allowance for the absence of observers in the southern hemisphere, and for the small probability that we see any considerable

[h] Laplace, 'Essai Philosophique,' p. 59. Todhunter's 'History,' pp. 491–94.

[i] Chamber's 'Astronomy,' 1st ed. p. 203.

[k] 'Philosophical Magazine,' 4th Series, vol. xl. p. 190; vol. xli. p. 44.

fraction of those which are in the neighbourhood of our system, we must accept Kepler's opinion, that there are more comets in the regions of space than fishes in the depths of the ocean. When like calculations are made concerning the numbers of meteors visible to us, it is astonishing to find that the number of meteors entering the earth's atmosphere in every twenty-four hours is probably not less than 400,000,000, of which 13,000 exist in every portion of space equal to that filled by the earth's globe.

Most serious fallacies may arise from overlooking the inevitable conditions under which the records of past events are brought to our notice. Thus it is only the durable objects manufactured by former races of men, such as flint implements, which can have come to our notice as a general rule. The comparative abundance of iron and bronze articles used by an ancient nation must not be supposed to be coincident with their comparative abundance in our museums, because bronze is far the more durable. There is always a prevailing fallacy that our ancestors built more strongly than we do, arising from the fact that the more fragile structures have long since crumbled away. It is thus that we have few or no relics of the habitations of the poorer classes among the Greeks or Romans, or in fact of any past race; for the temples, tombs, public buildings and mansions of the wealthier classes alone endure. There is an indefinite expanse of past events necessarily lost to us for ever, and we must generally look upon records or relics as exceptional in their character.

Exactly the same considerations apply to geological relics. We could not generally expect that animals would be preserved, unless as regards the bones, shells, strong integuments, or other hard and durable parts. All the infusoria and animals devoid of mineral framework must

probably have perished entirely, distilled perhaps into oils. It has been pointed out that the peculiar character of some extinct floras may be due to the unequal preservation of different families of plants. By various accidents, however, we may gain glimpses of a world that is usually lost to us—as by insects embedded in amber, the great mammoth preserved in ice, mummies, casts in solid material like that of the Roman soldier at Pompeii, and so forth.

We should also remember, that just as there may be conjunctions of the heavenly bodies that can have happened only once or twice in the period of history, so remarkable terrestrial conjunctions may take place. Great storms, earthquakes, volcanic eruptions, landslips, floods, irruptions of the sea may, or rather must, have occurred, events of such unusual magnitude and such extreme rarity that we can neither expect to witness them nor readily to comprehend their effects. It is a great advantage of the study of probabilities, as Laplace himself remarked, to make us mistrust the extent of our knowledge, and pay proper regard to the probability that events would come within the sphere of our observations.

Apparent Sequence of Events.

De Morgan has excellently pointed out[1] that there are no less than four modes in which one event may seem to follow or be connected with another, without being really so. These involve mental, sensual, and external causes of error, and I will briefly state and illustrate them.

Instead of A causing B, it may be *our perception of A that causes B.* Thus it is that prophecies, presentiments,

[1] 'Essay on Probabilities,' Cabinet Cyclopædia, p. 121.

and the devices of sorcery and witchcraft often work their own ends. A man dies on the day which he has always regarded as his last, from his own fears of the day. An incantation effects its purpose, because care is taken to frighten the intended victim, by letting him know his fate[m]. In all such cases the mental condition is the cause of apparent coincidence.

In a second class of cases, *the event A may make our perception of B follow, which would otherwise happen without being perceived.* Thus it was seriously believed as the result of investigation that more comets appeared in hot than cold summers. No account was taken of the fact that hot summers would be comparatively cloudless, and afford better opportunities for the discovery of comets[n]. Here the disturbing condition is of a purely external character. Certain ancient philosophers held that the moon's rays were cold-producing, mistaking the cold caused by radiation into space for an effect of the moon, which becomes visible at the time when the absence of clouds permits radiation to proceed.

In a third class of cases, *our perception of A may make our perception of B follow.* The event B may be constantly happening, but our attention may not be drawn to it except by our observing A. This case seems to be illustrated by the fallacy of the moon's influence on clouds. The origin of this fallacy is somewhat complicated. In the first place, when the sky is densely clouded the moon would not be visible at all; it would be necessary for us to see the full moon in order that our attention should be strongly drawn to the fact, and this would happen most often on those nights when the sky was cloudless. Mr. W. Ellis[o], moreover, has ingeniously

[m] Lubbock, 'Origin of Civilization,' p. 148.

[n] De Morgan's 'Essay,' p. 123.

[o] 'Philosophical Magazine,' 4th Series (1867), vol. xxxiv. p. 64.

pointed out that there is a general tendency for clouds to disperse at the commencement of night, which is the time when the full moon rises. Thus the change of the sky and the rise of the full moon are likely to attract attention mutually, and the coincidence in time suggests the relation of cause and effect. Mr. Ellis proves from the results of observations at the Greenwich Observatory that the moon possesses no appreciable power of the kind supposed, and yet it is remarkable that so acute and sound an observer as the late Sir John Herschel was convinced of the connection. In his 'Results of Observations at the Cape of Good Hope'[p], he mentions many evenings when a full moon occurred with a peculiarly clear sky.

There is yet a fourth class of cases, in which *B is really the antecedent event, but our perception of A, which is a consequence of B, may be necessary to bring about our perception of B.* There can be no doubt, for instance, that upward and downward currents are continually circulating in the lowest stratum of the atmosphere during the day-time; but owing to the transparency of the atmosphere we have no evidence of their existence until we perceive cumulous clouds, which are the consequence of such currents. In like manner an interfiltration of bodies of air in the higher parts of the atmosphere is probably in nearly constant progress, but unless threads of cirrous cloud indicate these motions we remain wholly ignorant of their occurrence[q]. The highest strata of the atmosphere are wholly imperceptible to us, except when rendered

[p] See 'Notes to Measures of Double Stars,' 1204, 1336, 1477, 1686, 1786, 1816, 1835, 1929, 2081, 2186, pp. 265, &c. See also Herschel's 'Familiar Lectures on Scientific Subjects, p. 147, and 'Outlines of Astronomy,' 7th ed. p. 285.

[q] Jevons, 'On the Cirrous Form of Cloud,' Philosophical Magazine, July, 1857, 4th Series, vol. xiv. p. 22.

luminous by auroral currents of electricity, or by the passage of meteoric stones.

There are many phenomena in meteorology and other similar sciences, in which some occurrences depend on others for their visibility. Thus in estimating the comparative numbers of meteors seen in different months of the year, it is essential to take account of the varying frequency of cloudy weather—or else of the different duration of the daylight which hides all but the most splendid meteors. Observations of the comparative frequency of various kinds of clouds will be complicated by the fact that dense rain clouds necessarily hide those more delicate cirrous clouds which appear in the higher parts of the atmosphere. Most of the visible phenomena of comets probably arise from some substance which, existing previously invisible, becomes condensed or electrified suddenly into a visible form. Sir John Herschel attempted to explain the production of comet tails in this manner by evaporation and condensation[r].

Negative Arguments founded on the Non-observation of Phenomena.

From what has been suggested in preceding sections, it will plainly appear that the non-observation of a phenomenon is not generally to be taken as proving its non-occurrence. As there are sounds which we cannot hear, rays of light which we cannot feel, indefinite multitudes of worlds which we cannot see, and infinite myriads of minute organisms of which not the most powerful microscope can give us a view, we must as a general rule interpret our experience in an affirmative sense only. Accordingly when inferences have been drawn from the non-occurrence of particular facts or objects, more ex-

[r] 'Astronomy,' 4th ed. p. 358.

tended and careful examination has often proved their falsity. Not many years since it was quite a well credited conclusion in geology that no remains of man were found in connexion with those of extinct animals, or in any deposit not actually at present in course of formation. Even Babbage accepted this conclusion as strongly confirmatory of the Mosaic accounts[8]. But when the opinion was yet universally held, flint implements had been found disproving any such conclusion, and overwhelming evidence of man's long continued existence has since been found. At the end of the last century when Herschel had searched the heavens with his powerful telescopes, there seemed little probability that planets yet remained unseen within the orbit of Jupiter. But on the first day of this century such an opinion was overturned by the discovery of Ceres, and more than a hundred other small planets have since been added to the lists of the planetary system.

The discovery of the Eozoon Canadense in strata of much greater age than any previously known to contain organic remains, has given a severe shock to many groundless opinions concerning the origin of organic forms; and the oceanic dredging expeditions, under Dr. Carpenter and Professor Wyville Thompson, have further disconcerted geologists by disclosing the continued existence of forms long supposed to be extinct. These and many other cases which might be quoted show the extremely unsafe character of negative inductions.

It must not be supposed that negative arguments are of no force and value. The earth's surface, for instance, has been sufficiently searched to render it highly improbable that any terrestrial animals of the size of a camel remain to be discovered. It is believed that no new large animal has been encountered in the last eighteen or twenty

[8] Babbage, 'Ninth Bridgewater Treatise,' p. 67.

centuries[t], and the probability that if existent they would have been seen, increases the probability that they do not exist. We may with somewhat less confidence discredit the existence of any large unrecognised fish, or sea animals, such as the alleged sea-serpent. But as we descend to forms of smaller size negative evidence loses weight from the less probability of our seeing smaller objects. Even the strong induction in favour of the four-fold division of the animal kingdom into Vertebrata, Annulosa, Mollusca, and Cœlenterata, may break down by the discovery of intermediate or anomalous forms. As civilisation spreads over the surface of the earth, and unexplored tracts are gradually diminished, negative conclusions will increase in force; but we require to learn much yet concerning the depths of the ocean, almost wholly unexamined as they are, and covering three-fourths of the earth's surface.

In geology there are a number of assertions to which considerable probability attaches on account of the large extent of the investigations already made, as, for instance, that true coal is found only in rocks of a particular geological epoch; that gold occurs in secondary and tertiary strata only in exceedingly small quantities[u], probably derived from the disintegration of earlier rocks.

In natural history negative conclusions are exceedingly treacherous and unsatisfactory. The utmost patience will not enable a microscopist or the observer of any living thing to watch the behaviour of the organism under all circumstances continuously for any great length of time. There is always a chance therefore that the critical act or change may take place when the observer's eyes are withdrawn. This certainly happens in some cases; for though

[t] Cuvier's 'Essay on the Theory of the Earth,' translation, p. 61, &c.

[u] Murchison's 'Siluria,' 1st ed. p. 432.

the fertilization of orchids by agency of insects is proved as well as any fact in natural history, Mr. Darwin has never been able by the closest watching to detect an insect in the performance of the operation. Mr. Darwin has himself, indeed, adopted one conclusion on purely negative evidence, namely that the Orchis pyramidalis and certain other orchidaceous flowers secrete no nectar. But his caution and unwearying patience in verifying the conclusion give an impressive lesson to the observer. For twenty-three consecutive days, as he tells us, he examined flowers, in all states of the weather, at all hours, in various localities. As the secretion in other flowers sometimes rapidly takes place and might happen at early dawn, that inconvenient hour of observation was specially adopted. Flowers of different ages were subjected to irritating vapours, to water, and every condition likely to bring on the secretion; and only after the invariable failure of this exhaustive inquiry was the barrenness of the nectaries assumed to be proved[x].

In order that a negative argument founded on the non-observation of an object shall have any considerable force, it must be shown to be probable that the object if existent would have been observed, and it is this probability which defines the value of the negative conclusion. The failure of astronomers to see the planet Vulcan, supposed by some to exist within Mercury's orbit, is no sufficient disproof of its existence. Similarly it would be very difficult, or even impossible, to disprove the existence of a second satellite of small size revolving round the earth. But if any person make a particular assertion, assigning place and time, then observation will either prove or disprove the alleged fact. Thus if it is true that when a French observer professed to have seen a planet on the sun's face, an observer in Brazil was carefully scrutinizing the sun and failed to see

[x] Darwin's 'Fertilization of Orchids,' p. 48.

it, we have a conclusive negative proof[y]. On this account, as it has been well said, false facts in science are more mischievous than false theories. A false theory is open to every person's criticism, and is ever liable to be judged by its accordance with facts. But a false or grossly erroneous assertion of a fact often stands in the way of science for a long time, because it may be extremely difficult or even impossible to prove the falsity of what has been once recorded.

In other sciences the force of a negative argument will often depend upon the number of possible alternatives which may exist. Thus it was long believed that the character or quality of a musical sound, as distinguished from its pitch must depend upon the form of the undulation, because no other cause of it had ever been suggested or was apparently possible. The truth of the conclusion was proved by Helmholtz, who applied a microscope to luminous points attached to the strings of various instruments, and thus actually observed the different modes of undulation[z].

In mathematics negative inductive arguments have seldom much force, because the possible forms of expression, or the possible combinations of lines and circles in geometry are quite unlimited in number. An enormous number of attempts were made to trisect the angle by the ordinary methods of Euclid's geometry, but their invariable failure did not establish the impossibility of the task. This was shown in a totally different manner, by proving that the problem involves an irreducible cubic equation to which there could be no corresponding plain geometrical solution[a]. This is a case of *reductio ad absurdum*, a form of argument of a totally different

[y] Chambers's 'Astronomy,' 1st ed. p. 31.

[z] 'Théorie Physiologique de la Musique', Paris, 1868, p. 113.

[a] Peacock, 'Algebra,' vol. ii. p. 344.

character. Similarly no number of failures to obtain a general solution of equations of the fifth degree would establish the impossibility of the task, but in an indirect mode, equivalent to a *reductio ad absurdum*, the impossibility is considered to be proved[b].

[b] Peacock, 'Algebra,' vol. ii. p. 359. Serret, 'Algèbra Supérieure,' 2nd ed. p. 289.

CHAPTER XIX.

EXPERIMENT.

WE now come to consider the great facilities which we enjoy for examining the possible combinations of properties and phenomena when objects are within our reach and capable of manipulation. We are said to *experiment*, when we bring substances together under various conditions of temperature, pressure, electric disturbance, molecular attraction, &c., and then record the changes observed.

If we denote by A a certain group of antecedent conditions, and by X a certain series of subsequent phenomena, our object will usually be to ascertain a law of the form $A = AX$, the meaning of which is that where A is X will happen, and we may sometimes rise to the still simpler and higher law $A = X$, meaning that where A is, and only where A is, X will happen (see vol. i. pp. 146, 149.)

The great object of the art of experiment is to ascertain exactly those circumstances or conditions which are requisite for the happening of any event X. Now the circumstances which might be enumerated as present in the very simplest experiment are very numerous, in fact almost infinite. Rub two sticks together and consider what would be an exhaustive statement of the conditions. There are the form, hardness, organic structure, and all the chemical qualities of the wood; the pressure and velocity of the rubbing; the temperature, pressure, and all the chemical qualities of the surrounding air; the proximity

of the earth with its attractive and electric properties; the temperature and other powers of the persons producing motion; the radiation from the sun, and to and from the sky; the electric excitement possibly existing in any overhanging cloud; even the positions of the heavenly bodies must be mentioned. Now on *à priori* grounds it is unsafe to assume that any one of these circumstances is without effect, and it is only on the results of experience that we can finally single out those precise conditions from which the observed heat of friction proceeds.

The great method of experiment consists in removing, one at a time, each of those conditions which may be imagined to have an influence on the result. Our object in the experiment of rubbing sticks is to discover the exact circumstances under which heat appears. Now the presence of air may be requisite; therefore prepare a vacuum, and rub the sticks in every respect as before, except that it is done *in vacuo*. If heat still appears we may say that air is not, in the presence of the other circumstances, a requisite condition. The conduction of heat from neighbouring bodies may be a condition. Prevent this by making all the surrounding bodies ice cold, which is practically what Davy aimed at in rubbing two pieces of ice together. If heat still appears we have eliminated another condition, and so we may go on until it becomes apparent that the expenditure of energy in the friction of two bodies is the sole condition of the production of heat.

The great difficulty of experiment arises from the fact that we must not assume an independence to exist among the conditions. Thus previous to experiment we have no right to say that the rubbing of two sticks will produce heat in the same way when air is absent as before. We may have heat produced in one way when air is present,

and in another when air is absent. The inquiry branches out into two lines, and we ought to try in both cases whether cutting off a supply of heat by conduction prevents its evolution in friction. Now the same branching out of the inquiry occurs with regard to every circumstance which enters into the experiment. Regarding only four circumstances, say A, B, C, D, we ought to test not only the combinations—

ABCD, ABC*d*, AB*c*D, A*b*CD, *a*BCD,

but we ought really to go through the whole of the combinations given in the fifth column of the Logical Abecedarium. The effect of the absence of each condition should be tried both in the presence and absence of every other condition, and every variety of selection of those conditions. Perfect and exhaustive experimentation would, in short, consist in examining natural phenomena in all their possible combinations and registering all relations between conditions and results which are found capable of existence. Experimentation would thus resemble the exclusion of contradictory combinations carried out in the Indirect Method of Inference (chapter vi. vol. i. p. 95), except that the exclusion of any combination is grounded not on prior logical premises, but on *à posteriori* results of actual trial.

The reader will readily perceive, however, that such exhaustive investigation is practically impossible, because the number of requisite experiments would be immensely great. Four circumstances only would require sixteen experiments; twelve circumstances would require 4096, and the number increases as the powers of two. The result is that the experimenter has to fall back upon his own tact and experience in selecting those variations which are most likely to yield him significant facts. It is at this point that logical rules and forms begin to fail in giving aid. The logical rule is—Try all possible com-

binations; but this being impracticable, the experimentalist necessarily abandons strict logical method, and trusts to his own insight. Analogy, as we shall afterwards see, gives some assistance, and attention will probably be concentrated on those kinds of conditions which have been found important in like cases. But we are now entirely in the region of probability, and the experimenter, while he is confidently pursuing what he thinks the right clue, may be entirely overlooking the one condition whose importance has been hitherto unsuspected. It is an impressive lesson, for instance, that Newton pursued all his exquisite researches on the spectrum unsuspicious of the fact that if he reduced the hole in the shutter to a narrow slit, all the mysteries of the bright and dark lines were within his grasp, provided of course that his prisms were sufficiently good to define the rays. In a similar manner we know not what slight alteration in the most familiar experiments may not open the way to realms of new discovery.

Many additional practical difficulties encumber the progress of the physicist. It is often impossible to alter one condition without altering others at the same time; and thus we may not get the pure effect of the condition in question. Some conditions may be absolutely incapable of alteration; others may be with great difficulty, or only in a certain degree, removable. A very treacherous source of error is the existence of unsuspected conditions, which we of course cannot remove except by accident. These difficulties we will shortly consider in succession.

It is often beautiful to observe how the alteration of a single circumstance conclusively explains a phenomenon. An excellent instance is found in Faraday's investigation of the behaviour of Lycopodium spores scattered on a vibrating plate. It was observed that these minute spores collected together at the points of greatest motion, whereas

sand and all heavy particles collect at the nodes, where motion is least. But it happily occurred to Faraday to try the experiment in the exhausted receiver of an air-pump, and it was then found that the light powder behaved exactly like heavy powder. A conclusive proof was thus obtained that the presence of air was the condition of importance, doubtless because it was thrown into eddies by the motion of the plate, and thus carried the Lycopodium to the points of greatest agitation. Sand was too heavy to be thus carried by the air.

Exclusion of Indifferent Circumstances.

From what has been already said it will be apparent that in the investigation of any new phenomenon the detection and exclusion of indifferent circumstances is a work of great importance, because it allows the concentration of attention upon circumstances which may contain the principal condition. There will always be a multitude of things which we are only too ready to neglect, but many beautiful instances may be given where all the most obvious circumstances have been shown to have no part in the production of a phenomenon. Every person would suppose that the peculiar colours of mother-of-pearl were due to the chemical qualities of the substance. Much trouble might have been spent in following out that notion by comparing the chemical qualities of various iridescent substances. But Brewster accidentally took an impression from a piece of mother-of-pearl in a cement of resin and bees'-wax, and finding the colours repeated upon the surface of the wax, proceeded to take other impressions in balsam, fusible metal, lead, gum arabic, isinglass, &c., and always found the iridescent colours the same. He thus proved that the chemical nature is wholly a matter

of indifference, and the form of the surface is the condition of such colours[a].

Nearly the same may be said of the colours exhibited by thin plates and films. The rings and lines of colour will be of the same character whatever may be the nature of the substance; nay, a void space, such as a crack in glass, would produce them even though the air were withdrawn by an air-pump. The conditions are simply the existence of two reflecting surfaces separated by a very small space, though it should be added that the refractive index of the intervening substance has some influence on the exact nature of the colour produced at any point.

When a ray of light passes close to the edge of an opaque body, a portion of the light appears to be bent towards it, and produces coloured fringes within the shadow of the body. Newton attributed this inflexion of light to the attraction of the opaque body for the supposed particles of light, although he was aware that the nature of the surrounding medium, whether air or other pellucid substance, exercised no apparent influence on the phenomena. Gravesande proved however that the character of the fringes is exactly the same, whether the body be dense or rare, compound or elementary. A wire has exactly the same effect as a hair of the same thickness. Even the form of the obstructing edge was subsequently shown to be a matter of indifference by Fresnel, and the interference spectrum, or the spectrum seen when light passes through a fine grating is absolutely the same whatever be the form or chemical nature of the bars forming the grating. Thus it appears that the stoppage of a portion of a beam of light is the sole necessary condition for the diffraction or inflexion of light; and the phenomenon is shown to bear no analogy to the reflection and refraction of light,

[a] 'Treatise on Optics,' by Sir D. Brewster, Cabinet Cyclopædia, p. 117.

in which the form and nature of the substance are all important.

It is interesting to observe how carefully Newton, in his researches on the spectrum, observed and proved the indifference of many circumstances by actual trial. He says[b]: 'Now the different magnitude of the hole in the window-shut, and different thickness of the prism where the rays passed through it, and different inclinations of the prism to the horizon, made no sensible changes in the length of the image. Neither did the different matter of the prisms make any: for in a vessel made of polished plates of glass cemented together in the shape of a prism, and filled with water, there is the like success of the experiment according to the quantity of the refraction.' But in the latter statement, as I shall afterwards remark (vol. ii. p. 42), Newton assumed an indifference which does not exist, and fell into an unfortunate mistake.

In the science of sound it is shown that the pitch of a sound depends solely upon the number of impulses in a second, and the material exciting those impulses is a matter of perfect indifference. Thus whatever medium, whether air or water, or any gas or liquid, be forced into the Siren, the sound produced is the same; and the material of which an organ-pipe is constructed does not at all affect the pitch of its sound.

In the science of statical electricity it is an important circumstance that the interior of a conducting body is a matter of indifference, resting in a neutral state, while the change is confined to the conducting surface. A hollow sphere takes exactly the same charge as a solid sphere of metal.

Some of Faraday's most elegant and successful researches were devoted to the exclusion of conditions

[b] 'Opticks,' 3rd edit. p. 25.

which previous experimenters had thought essential for the production of electrical phenomena. Davy asserted that no known fluids, except such as contain water, could be made the medium of connexion between the poles of a battery; and some chemists believed that water was an essential agent in electro-chemical decomposition. Faraday gives abundant experiments to show that other fluids allow of electrolysis, and attributes the erroneous opinion to the very general use of water as a solvent, and its presence in most natural bodies[c]. It was, in fact, upon purely negative (vol. ii. p. 16) and weak evidence that the opinion had been founded.

Many experimenters attributed peculiar and even mysterious powers to the poles of a battery, likening them to magnets, which, by their attractive powers, tear apart the elements of a substance. By a most beautiful series of experiments[d], Faraday proved conclusively that, on the contrary, the substance of the poles is of no importance, being merely the path through which the electric force reaches the liquid acted upon. Poles of water, charcoal, and many diverse substances, even air itself, produced similar results, or if the chemical nature of the pole entered at all into the question, it was as a disturbing agent.

It is a most essential part of the theory of gravitation that the proximity of other attracting particles is wholly without effect upon the attraction existing between any two molecules. Two pound weights weigh as much together as they do separately. Every pair of molecules in the world have, as it were, a private communication, apart from their relations to all other molecules. Another undoubted result of experience pointed out by Newton[e] is that the weight of a body does not in the least depend

[c] 'Experimental Researches in Electricity,' vol. i. pp. 133, 134.

[d] Ibid. vol. i. pp. 127, 162, &c.

[e] 'Principia,' bk. iii. Prop. vi. Corollary i.

upon its form or texture. It may be added that the temperature, electric condition, pressure, state of motion, chemical qualities, and all other circumstances concerning matter, except its mass, are indifferent as regards its gravitating power.

As natural science progresses, physicists gain a kind of insight and tact in judging what qualities of a substance are likely to be concerned in any class of phenomena. The physical astronomer treats matter in one point of view, the chemist in another, and the students of physical optics, sound, mechanics, electricity, &c., make a fair division of the qualities among them. But errors will arise if too much confidence be placed in this independence of various kinds of phenomena, so that it is desirable from time to time, especially when any unexplained discrepancies come into notice, to question the indifference which is assumed to exist, and to test its real existence by appropriate experiments.

Simplification of Experiments.

One of the most requisite precautions in experimentation is to vary only one circumstance at a time, and to maintain all other circumstances rigidly unchanged. There are two distinct reasons for this rule, the first and most obvious being that if we vary two conditions at a time, and find some effect, we cannot tell whether the effect is due to one or the other, or to both jointly. A second reason is that if no effect ensues we cannot safely conclude that either of them is indifferent; for the one may have neutralized the effect of the other. In our logical formulæ, A (B + b) is identical with A (see vol. i. p. 112), and B may be indifferently present or absent; but A (BC + bc) is not identical with A, and none of our logical processes enabled us to make the reduction.

If we want to prove that oxygen is necessary to life, we must not put a rabbit into a vessel from which the oxygen has been exhausted by a burning candle. We should then have not only an absence of oxygen, but an addition of carbonic acid, which may have been the destructive agent. For a similar reason Lavoisier avoided the use of atmospheric air in experiments on combustion, because air was not a simple substance, and the presence of nitrogen might impede or even alter the effect of oxygen. As Lavoisier expressly remarks[f], 'In performing experiments, it is a necessary principle, which ought never to be deviated from, that they be simplified as much as possible, and that every circumstance capable of rendering their results complicated be carefully removed.' It has also been well said by Cuvier[g] that the method of physical inquiry consists in isolating bodies, reducing them to their utmost simplicity, and in bringing each of their properties separately into action, either mentally or by experiment.

The electro-magnet has been of the utmost service in the investigation of the magnetic properties of matter, by allowing of the production or removal of a most powerful magnetic force without disturbing any of the other arrangements of the experiment. Many of Faraday's most valuable experiments would have been frustrated had it been necessary to introduce a heavy permanent magnet, which could not be suddenly moved without shaking the whole apparatus, disturbing the air, producing currents by differences of temperature, &c. The electro-magnet is perfectly under control, and its influence can be brought into action, reversed, or stopped by merely touching a button. Thus Faraday was enabled to prove the rotation of the plane of circular polarized light by the fact that a certain light ceased to be visible when the electric current

[f] Lavoisier's 'Chemistry,' translated by Kerr, p. 103.

[g] Cuvier's 'Animal Kingdom,' introduction, pp. 1, 2.

of the magnet was cut off, and *vice versâ* the light appeared when the current was re-made. 'These phenomena,' he says, 'could be reversed at pleasure, and at any instant of time, and upon any occasion, showing a perfect dependence of cause and effect[h].'

Another elegant experiment by Faraday illustrates the maintainance of similar conditions. He proved that liquids may conduct electricity when solids will not, by putting the poles of a battery in melted nitre, when a strong current was shown to exist by the galvanometer. But as soon as the nitre was allowed to solidify, the current ceased. Everything else remaining the same, the current existed when the nitre was liquid, and not when the nitre was solid[i].

It was Newton's omission to obtain the solar spectrum under the simplest conditions which prevented him from discovering the dark lines. Using a broad beam of light which had passed through a round hole or a triangular slit, he obtained a brilliant spectrum, but one in which many different coloured rays overlapped each other. In the recent history of the science of the spectrum, one main difficulty has consisted in the mixture of the lines of several different substances, which are usually to be found in the light of any flame or spark. It is seldom possible to obtain the light of any element in a perfectly simple manner. Angström greatly advanced this branch of science, by examining the light of the electric spark when formed between poles of various metals, and in the presence of various gases. By varying the pole alone, or the gaseous medium alone, he was able to discriminate correctly between the lines due to the metal and those due to the surrounding gas[k].

h 'Experimental Researches in Electricity,' vol. iii. p. 4.

i 'Life of Faraday,' vol. ii. p. 24.

k 'Philosophical Magazine,' 4th Series, vol. ix. p. 327.

Failure in the Simplification of Experiments.

In some cases it seems to be impossible to carry out the rule of varying one circumstance at a time. When we attempt to obtain two instances or two forms of experiment in which a single circumstance shall be present or absent, it may be found that this single circumstance entails one or more others. Benjamin Franklin's experiment concerning the comparative absorbing powers of different colours is well known. 'I took,' he says, 'a number of little square pieces of broadcloth from a tailor's pattern card, of various colours. They were black, deep blue, lighter blue, green, purple, red, yellow, white, and other colours and shades of colour. I laid them all out upon the snow on a bright sunshiny morning. In a few hours, the black being most warmed by the sun, was sunk so low as to be below the stroke of the sun's rays; the dark blue was almost as low; the lighter blue not quite so much as the dark; the other colours less as they were lighter. The white remained on the surface of the snow, not having entered it at all.' This is a very elegant and apparently simple experiment; but when Leslie had completed his series of researches upon the nature of heat, he came to the conclusion that the colour of a surface has very little effect upon the radiating power, the mechanical nature of the surface appearing to be more influential. He remarks[1] that 'the question is incapable of being positively resolved, since no substance can be made to assume different colours without at the same time changing its internal structure.' More recent investigation has shown that the subject is one of considerable complication, because the absorptive power of a surface may be different according to the character of the rays which fall upon it;

[1] 'Inquiry into the Nature of Heat,' p. 95.

but there can be no doubt as to the acuteness with which Léslie points out the difficulty. In Well's investigations concerning the nature of dew, we have, again, very complicated conditions. If we expose plates of various material, such as rough iron, glass, polished metal, to the midnight sky, they will be dewed in various degrees; but since these plates differ both in the nature of the surface and the conducting power of the material, it would not be plain whether one or both circumstances were of importance. We avoid this difficulty by exposing the same material polished or varnished, so as to present different conditions of surface[m]; and again by exposing different substances with the same kind of surface.

When we are quite unable to isolate circumstances we must resort to the procedure described by Mr. J. S. Mill under the name of the Joint Method of Agreement and Difference. We must collect as many instances as possible in which a given circumstance produces a given result, and as many as possible in which the absence of the circumstance is followed by the absence of the result. To adduce his example, we cannot experiment upon the cause of double refraction in Iceland spar, because we cannot alter its crystalline condition without altering it altogether, nor can we find substances exactly like calc spar in every circumstance except one. We can only resort therefore to the method of comparing together all known substances which have the property of doubly-refracting light, and we find that they agree in being crystalline[n]. This indeed is nothing but an ordinary process of perfect or probable induction, already partially described, and to be further discussed under the subject of Classification. It may be added, however, that the subject does admit of

[m] Herschel, 'Preliminary Discourse on the Study of Natural Philosophy,' p. 161.

[n] 'System of Logic,' bk. III. chap. viii. § 4, 5th. ed. vol. i. p. 433.

perfect experimental treatment, since glass, when strongly compressed, and so long only as it is compressed in one direction, becomes capable of doubly-refracting light, and as there is probably no alteration in the glass but change of elasticity, we learn that the power of double refraction is very probably due to a difference of elasticity in different directions.

Removal of Usual Conditions.

One of the great objects of experiment is to enable us to judge of the behaviour of substances under conditions widely different from those which prevail upon the surface of the earth. We live in an atmosphere which does not vary beyond certain narrow limits in temperature or pressure. Many of the powers of nature, such as gravity, which constantly act upon us, are of almost fixed amount. Now it will afterwards be shown that we cannot apply a quantitative law to circumstances much different from those in which it was observed, without considerable risk of error. In the other planets, the sun, the stars, or remote parts of the Universe, the conditions of existence must often be widely different from what we commonly experience here. Hence our knowledge of nature must remain very restricted and hypothetical, unless we can subject substances to very unusual conditions by suitable experiments.

The electric arc is an invaluable means of exposing metals or other conducting substances to the highest known temperature. By its aid we learn not only that all the metals can be vaporized, but that they all give off distinctive rays of light. At the other extremity of the scale, the intensely powerful freezing mixture devised by Faraday, consisting of solid carbonic acid and ether mixed in vacuo, enables us to observe the nature of substances at temperatures immensely below any we meet with naturally on the earth's surface.

We can hardly realize now the importance of the invention of the air-pump, previous to which it was exceedingly difficult to make any experiment except under the ordinary pressure of the atmosphere. The Torricellian vacuum had been employed by the philosophers of the Accademia del Cimento to show the behaviour of water, smoke, sound, magnets, electric substances, &c., in vacuo, but their experiments were often unsuccessful from the difficulty of excluding air[o].

Among the most constant circumstances under which we live is the force of gravity, which does not vary, except by a slight fraction of its amount, in any part of the earth's crust or atmosphere to which we can attain. Now this force is often sufficient to overbear and disguise various actions; for instance, the mutual gravitation of small bodies. It was an interesting experiment of Plateau to withdraw substances from the action of gravity by suspending them in liquids of exactly the same specific gravity. Thus a quantity of oil poured into the middle of a suitable mixture of alcohol and water, assumes a spherical shape which, on being made to rotate, becomes spheroidal, and then successively separates into a ring and a group of spherules. Thus we have at least an illustration of the mode in which the planetary system may have been produced[p], though it is to be remembered that the extreme difference of scale prevents our arguing with confidence from the experiment to the conditions of the nebular theory.

It is possible that the so-called elements are elementary only to us, because we are restricted to temperatures at which they are fixed. Lavoisier carefully defined an element as a substance which cannot be decomposed *by*

o 'Essayes of Natural Experiments made in the Accademia del Cimento.' Englished by Richard Waller, 1684, p. 40, &c.

p Plateau, Taylor's 'Scientific Memoirs,' vol. iv. pp. 16–43.

any known means; but it seems almost certain that some series of elements, for instance Iodine, Bromine, and Chlorine, are really compounds of a simpler substance. We must doubtless look to the production of intensely high temperatures, as yet quite beyond our means, for the decomposition of these so-called elements. But it may very possibly be found that, in this age and part of the universe, the dissipation of energy has so far proceeded that there are no sources of heat left to us sufficiently intense to effect the decomposition of the supposed elements.

Interference of Unsuspected Conditions.

It may often happen that we are not aware of all the conditions under which our researches are made. Some substance may be present or some power may be in action, which escapes the most vigilant examination. Not being aware of its existence, we are of course unable to take proper measures to exclude it, and thus determine the share which it may have in the results of our experiments. There can be little doubt that the alchemists were often misled and encouraged in their vain attempts by the unsuspected presence of traces of gold and silver in the substances they proposed to transmute. Lead, as drawn from the smelting furnace, almost always contains some silver, and gold is associated with many other metals. Thus small quantities of noble metal would often appear as the result of experiment and raise delusive hopes.

In more than one case the unsuspected presence of common salt in the air has caused great trouble. In the early experiments on electrolysis it was found that, when water was decomposed, an acid and an alkali were produced at the poles, together with oxygen and hydrogen. In the absence of any other explanation for this singular result, some chemists rushed to the conclusion

that electricity must have the power of *generating* acids and alkalis, and one chemist thought he had discovered a new substance called *electric acid*. But Davy proceeded to a systematic investigation of the circumstances, by varying the conditions. Changing the glass vessel for one of agate or gold, he found that far less alkali was produced; excluding impurities by the use of very carefully distilled water, he found that the quantities of acid and alkali were still further diminished; and having thus obtained a clue to the cause he completed the exclusion of impurities by avoiding contact with his fingers, and by placing the apparatus under an exhausted receiver, no acid or alkali being then detected. It would be difficult to meet with a more elegant or successful case of the detection of a condition previously unsuspected [q].

It is highly remarkable that the presence of common salt in the air, proved to exist by Davy, nevertheless continued a stumbling-block in the science of spectrum analysis, and probably prevented men, such as Brewster, Herschel, and Talbot, from anticipating by thirty years the discoveries of Bunsen and Kirchhoff. As I have elsewhere pointed out[r], the utility of the spectrum was known in the middle of the last century to Thomas Melvill, a talented Scotch physicist, who died at the early age of 27 years[s]. But Melvill was struck in his examination of various coloured flames by the extraordinary predominance of homogeneous yellow light, which was due to some circumstance escaping his attention. Wollaston and

[q] 'Philosophical Transactions,' [1826] vol. cxvi. pp. 388, 389. Works of Sir Humphry Davy, vol. v. pp. 1–12.

[r] 'National Review,' July, 1861, p. 13.

[s] His published works are contained in 'The Edinburgh Physical and Literary Essays,' vol. ii. p. 34; 'Philosophical Transactions,' [1753] vol. xlviii. p. 261; see also Morgan's Paper in 'Philosophical Transactions,' [1785] vol. lxxv. p. 190.

Fraunhofer were equally struck by the prominence of the yellow line in the spectrum of nearly every kind of light. Talbot expressly recommended the use of the prism for detecting the presence of substances by what we now call spectrum analysis, but he found that all substances, however different the light they yielded in other respects, were identical as regards the production of yellow light. Talbot knew that the salts of soda all gave this coloured light, but in spite of Davy's previous difficulties with salt in electrolysis, it did not occur to him to assert that where the light is, there the sodium must be. He suggested water as the most likely source of the yellow light, because of its usual presence; but even substances which were apparently devoid of water gave the very same yellow light[t]. Brewster and Herschel both experimented upon flames almost at the same time as Talbot, and Herschel unequivocally enounced the principle of spectrum analysis[u]. Nevertheless Brewster, after numerous experiments attended with great trouble and disappointment, found that yellow light might be obtained from the combustion of almost any substance. It was not until 1856 that Professor W. Swan discovered that an almost infinitesimal quantity of sodium chloride, say a millionth part of a grain, was sufficient to tinge a flame of a bright yellow colour. The universal diffusion of the salts of sodium, joined to this unique light-producing power, was thus shown to be the unsuspected circumstance which had destroyed the confidence of all previous experimenters in the use of the prism. Some references concerning the history of this curious point are given below[x].

[t] 'Edinburgh Journal of Science,' vol v. p. 79.

[u] 'Encyclopædia Metropolitana,' article *Light*, § 524; Herschel's 'Familiar Lectures,' p. 266.

[x] Talbot, 'Philosophical Magazine,' 3rd Series, vol. ix. p. 1 (1836); Brewster, 'Transactions of the Royal Society of Edinburgh,' [1823] vol.

In the science of radiant heat, early inquirers were led to the conclusion that radiation proceeded only from the surface of a solid, or from a very small depth below it. But they happened to experiment upon surfaces covered by coats of varnish, which is highly athermanous or opaque to heat. Had they properly varied the character of the surface, using a highly diathermanous substance like rock salt, they would have obtained very different results[y].

One of the most extraordinary instances of an erroneous opinion due to overlooking interfering agents is that concerning the increase of rainfall near to the earth's surface. More than a century ago it was observed that rain-guages placed upon church steeples, house tops, and other elevated places, gave considerably less rain than if they were on the ground, and it has very recently been shown that the variation is most rapid in the close neighbourhood of the ground[z]. All kinds of theories have been started to explain this phenomenon; but I have attempted to show[a] that it is simply due to the interference of wind, which deflects more or less rain from all the guages which are at all exposed to it.

The great magnetic power of iron renders it a constant source of disturbance in all magnetic experiments. In building a magnetic observatory great care must therefore be taken that no iron is employed in the construction, and that no masses of iron are near at hand. In some cases magnetic observations have been seriously disturbed by the existence of masses of iron ore in the neighbourhood. In Faraday's experiments upon feebly magnetic or diamag-

ix. pp. 433, 455; Swan, ibid. [1856] vol. xxi. p. 411.; 'Philosophical Magazine,' 4th Series, vol. xx. p. 173, [Sept. 1860]; Roscoe, 'Spectrum Analysis,' Lecture III.

y Stewart, 'Elementary Treatise on Heat,' p. 192.

z British Association, Liverpool, 1870. 'Report on Rainfall,' p. 176.

a 'Philosophical Magazine,' Dec. 1861, 4th Series, vol. xxii. p. 421.

netic substances he took the greatest precautions against the presence of any disturbing substance in the copper wire, wax, paper, and other articles used in suspending the test objects. It was his invariable custom to try the effect of the magnet upon the apparatus in the absence of the object of experiment, and without this preliminary trial no confidence could be placed in the results[b]. Tyndall has also employed the same mode for testing the freedom of electro-magnetic coils from iron, and was thus enabled to obtain them devoid of any cause of disturbance[c]. It is well worthy of notice that in the very infancy of the science of magnetism, the acute experimentalist Gilbert correctly accounted for the opinion existing in his day that magnets would attract silver, by pointing out that the silver contained iron[d].

Even when we are not aware by previous experience of the probable presence of a special disturbing agent, we ought not to assume the absence of unsuspected interference. If, then, an experiment is of really high importance, so that any considerable branch of science rests upon it, we ought to try it again and again, in as varied conditions as possible. We should intentionally disturb the apparatus in various ways, so as if possible to hit by accident upon any peculiar weak points. Especially when our results are more regular and accordant than we have fair grounds for anticipating, ought we to suspect some peculiarity in the apparatus which causes it to measure some other phenomenon than that in question, just as Foucault's pendulum almost invariably indicates the revolution of the axes of its own elliptic path instead of the revolution of the globe.

It was in this cautious spirit that Baily acted in his

[b] 'Experimental Researches in Electricity,' vol. iii. p. 84, &c.
[c] 'Lectures on Heat,' p. 21. [d] Gilbert, 'De Magnete.'

splendid experiments on the density of the earth. The accuracy of his results entirely depended upon the elimination of all disturbing influences, so that the oscillation of his torsion balance should depend on gravity alone. Hence he varied the apparatus in many ways, changing the small balls subject to attraction, changing the connecting rod, and the means of suspension. He observed the effect of artificial disturbances, such as the presence of visitors, the occurrence of violent storms, &c., and as no real alteration was produced in the results, he confidently attributed them to gravity[e].

Newton would probably have discovered the mode of constructing achromatic lenses, but for the unsuspected effect of some sugar of lead which he is supposed to have dissolved in the water of a prism. He tried, by means of a glass prism combined with a water prism, to produce dispersion of light without refraction, and if he had succeeded there would have been an obvious mode of producing refraction without dispersion. His failure is supposed to be due to his adding lead acetate to the water for the purpose of increasing its refractive power, the lead having a high dispersive power which frustrated his purpose[f]. Judging from Newton's remarks, in the 'Philosophical Transactions,' it would appear as if he had not, without many unsuccessful trials, despaired of the construction of achromatic glasses[g].

The Academicians of Cimento, in their early and ingenious experiments upon the vacuum, were often misled by the mechanical imperfections of their apparatus. They concluded that the air had nothing to do with the pro-

[e] Baily, 'Memoirs of the Royal Astronomical Society,' vol. xiv. pp. 29–30.

[f] Grant's 'History of Physical Astronomy,' p. 531.

[g] 'Philosophical Transactions,' abridged by Lowthorp, 4th edition, vol. i. p. 202.

duction of sounds, evidently because their vacuum was not sufficiently perfect[h]. Otto von Guericke fell into a like mistake in the use of his newly-constructed air-pump, doubtless from the unsuspected presence of air sufficiently dense to convey the sound of the bell[i].

It is hardly requisite to point out that the doctrine of spontaneous generation is due to the unsuspected presence of germs, even after the most careful efforts to exclude them[k], and in the case of many diseases, both of animals and plants, germs which we have no means as yet of detecting and examining, are doubtless the active cause. It has long been a subject of dispute, again, whether the plants which spring up from newly turned land grow from seeds long buried in that land, or from seeds brought by the wind. Argument is unphilosophical when direct trial can readily be applied; for by turning up some old ground, and covering a portion of it with a glass case, the conveyance of seeds by the wind can be entirely prevented, and if the same plants appear within and without the case, it will become clear that the seeds are in the earth. By gross oversight some experimenters have thought before now that crops of rye had sprung up where oats had been sown[l].

Blind or Test Experiments.

Every correct and conclusive experiment necessarily consists in the comparison of results between two different combinations of circumstances. To give a fair probability

h 'Essayes of Natural Experiments,' &c. Englished by Richard Waller, p. 50.

i Whewell, 'History of the Inductive Sciences,' 3rd edition, vol. ii. p. 246.

k Berkeley's 'Introduction to Cryptogamic Botany,' pp. 258, 259.

l Dr. Weissenborn, in the new series of 'Magazine of Natural History,' vol. i. p. 574, quoted in 'Vestiges of Creation,' 2nd edition, p. 222.

that A is the cause of X, I must maintain invariable all surrounding objects and conditions, and I must then show that where A is X is, and where A is not X is not. Now this cannot really be accomplished in a single trial. If, for instance, a chemist places a certain suspected substance in the Marsh's test apparatus, and finds that it gives a small deposit of metallic arsenic, he cannot be sure that the arsenic really proceeded from the suspected substance; for the impurity of the zinc or sulphuric acid might have been the cause of its appearance. It is therefore the practice of chemists to make what they call a *blind experiment*, that is to try whether arsenic appears in the absence of the suspected substance. The same precaution ought to be taken in all important analytical operations. Indeed, it is not merely a precaution, it is an essential part of any experiment. If the blind trial be not made, the chemist merely assumes that he knows what would happen. Whenever we assert that because A and X are found together A is the cause of X, we imply and assume that if A were absent X would be absent. But wherever it is possible, we ought clearly not to leave this as a mere assumption, or even as a matter of inference. Experience is ultimately the basis of all our inferences, but if we can with care bring immediate experience to bear upon the point in question we should not trust to anything more remote and liable to error. When Faraday examined the magnetic properties of the bearing apparatus, in the absence of the substance to be experimented on, he really made a blind experiment (see vol. ii. p. 41).

We ought also, whenever we can, to test the sufficiency and accuracy of any method of experiment by introducing known amounts of the substance or force to be detected. Thus a new analytical process for the quantitative estimation of an element should be tested by performing it

upon a mixture compounded so as to contain a known quantity of that element. The accuracy of the gold assay process greatly depends upon the precaution of assaying alloys of gold of exactly known composition[m]. Gabriel Plattes' works give evidence of much scientific spirit, and when discussing the supposed merits of the divining rod for the discovery of subterranean treasure, he sensibly suggests that the rod should be tried in places where veins of metal are known to exist, and, we might add, known not to exist[n].

Negative Results of Experiment.

When we pay proper regard to the imperfection of all measuring instruments and the possible minuteness of effects, we shall see much reason for interpreting with caution the negative results of experiments. We may fail to discover the existence of an expected effect, not because that effect is really non-existent, but because it is of an amount inappreciable to our senses, or confounded with other effects of much greater amount. As in fact there is no limit on *à priori* grounds to the smallness of a phenomenon, we can never, on the grounds of a single experiment, prove the non-existence of a supposed effect. We are always at liberty to assume that a certain amount of effect might have been detected by greater delicacy of measurement. We cannot safely affirm that the moon has no atmosphere at all. We may doubtless show that the atmosphere, if present, is less dense than the air in the so-called vacuum of an air-pump, as did Du Sejour. It is equally impossible to prove that gravity occupies *no time* in transmission. Laplace indeed ascertained that the velocity of propagation of the influence was at least fifty

[m] Watts, 'Dictionary of Chemistry,' vol. ii. pp. 936, 937.

[n] 'Discovery of Subterraneal Treasure,' London, 1639, p. 48.

million times greater than that of light[o]; but it does not really follow that it is instantaneous; and were there any means of detecting the action of one star upon another exceedingly distant star, we might possibly find an appreciable interval occupied in the transmission of the gravitating impulse. Newton could not demonstrate the absence of all resistance to matter moving through space, or the adamantine basis of light; but he ascertained by one of the most beautiful experiments with the pendulum, elsewhere more fully described (vol. ii. p. 55), that if such resistance existed, it was in amount less than one five-thousandth part of the external resistance of the air[p].

Innumerable incidents in the history of science tend to show that phenomena, which one generation has failed to detect, may become accurately known to a succeeding generation. The compressibility of water which the Academicians of Florence could not prove, because at a low pressure the effect was too small to perceive, and at a high pressure the water oozed through their silver vessel[q], has now become the subject of exact measurements and precise calculation. Independently of Newton, Hooke entertained very remarkable notions concerning the nature of gravitation. In this and other subjects he showed, indeed, a genius for experimental investigation which would have placed him in the first rank in any other age than that of Newton. He correctly conceived that the force of gravity would decrease as we receded from the centre of the earth, and he boldly attempted to prove it by experiment. Having exactly counterpoised two weights in the scales of a balance, or rather one weight against another weight and a long piece of fine cord, he removed his

o Laplace, 'System of the World,' transl. by Harte, vol. ii. p. 322.

p 'Principia,' bk. II. sec. 6, Prop. xxxi. Motte's translation, vol. ii. p. 108.

r 'Essayes of Natural Experiments,' &c. p. 117.

balance to the top of the Dome of St. Paul's, and tried whether the balance remained in equilibrium after one weight was allowed to hang down to a depth of 240 feet. No difference could be perceived when the weights were at the same and at different levels, but Hooke rightly held that the failure arose from the insufficient difference of height. He says, 'Yet I am apt to think some difference might be discovered in greater heights[r].' The radius of the earth being about 20,922,000 feet, we can now readily calculate from the known law of gravity that a height of 240 would not make a greater difference than one part in 40,000 of the weight. Such a difference would doubtless be inappreciable in the balances of that day, though it could readily be detected by balances now frequently constructed. Again, the mutual gravitation of bodies at the earth's surface is so small that Newton appears to have made no attempts to demonstrate its existence experimentally, merely remarking that it was too small to fall under the observation of our senses[s]. It has since been successfully detected and measured by Cavendish, Baily and others.

The smallness of the quantities which we can now observe is often very astonishing. A balance will weigh to one millionth part of the load or less. Sir Joseph Whitworth can measure to the one millionth part of an inch. A rise of temperature of the 8800th part of a degree centigrade has been detected by Dr. Joule. The spectroscope can reveal the presence of the one 180,000,000th part of a grain of soda, and the sense of smell can probably feel the presence of a far less quantity of odorous matter[t]. We must nevertheless remember that effects of indefinitely

[r] Hooke's 'Posthumous Works,' p. 182.

[s] 'Principia,' bk. III, Prop. vii. Corollary 1.

[t] Keill's 'Introduction to Natural Philosophy.' 3rd ed., London, 1733, pp. 48–54.

less amount than these must exist, and we should state our negative result with corresponding caution. We can only disprove the existence of a quantitative phenomenon by showing deductively, from the laws of nature, that if present it would amount to a perceptible quantity. As in the case of other negative arguments (vol. ii. p. 19) we must demonstrate that the effect would appear, where it is by experiment found not to appear.

Limits of Experiment.

It will be obvious that there are many operations of nature which we are quite incapable of imitating in our experiments. Our object is to study the conditions under which a certain effect is produced; but one of those conditions may involve a great length of time. There are instances on record of experiments extending over five or ten years, and even over a large part of a lifetime; but such intervals of time are almost nothing to the time during which nature may have been at work. The contents of a mineral vein in Cornwall may have been undergoing gradual change for a million years or more. All metamorphic rocks have doubtless endured high temperature and enormous pressure for almost inconceivable periods of time, so that chemical geology is generally beyond the scope of experiment.

Arguments have been continually brought against Darwin's theory, founded upon the absence of any clear instance of the production of a new species. During an historical period of perhaps four thousand years, no animal, it is said, has been so much domesticated as to become different in species. It might as well be argued, as it seems to me, that no geological changes are taking place, because no new mountain has risen in Great Britain within the memory of man. Our actual experience of

geological changes is like a mere point in the infinite progression of time. When we know that rain water falling on limestone will carry away a minute portion of the rock in solution, we do not hesitate to multiply that quantity by millions and millions, and assert that in course of time a mountain may be dissolved away. We have actual experience concerning the rise of land in some parts of the globe and its fall in others to the extent of some feet. Do we hesitate to infer what may thus be done in course of geological ages? As Gabriel Plattes long ago remarked, 'The sea never resting, but perpetually winning land in one place and losing in another, doth shew what may be done in length of time by a continual operation, not subject unto ceasing or intermission'[u]. The action of physical circumstances upon the forms and characters of animals by natural selection is subject to exactly the same remarks. As regards animals living in a state of nature the change of circumstances which can be ascertained to have occurred is so indefinitely slight, that we could not expect to observe any change in those animals whatever. Nature has made no experiment at all for us within historical times. Man, however, by taming and domesticating dogs, cats, horses, oxen, &c., has made considerable change in their circumstances, and we find considerable change also in their forms and character. Supposing the state of domestication to continue unchanged, these new forms would continue permanent so far as we know, and in this sense they are permanent. Thus the arguments against Darwin's theory, founded on the non-observation of natural changes within the historical period, are of the weakest character, being purely negative.

[u] 'Discovery of Subterraneal Treasure,' 1639, p. 52.

CHAPTER XX.

METHOD OF VARIATIONS.

EXPERIMENTS may be of two kinds: experiments of simple fact, and experiments of quantity. In the first class of experiments we combine certain conditions, and wish to ascertain whether or not a certain effect of any quantity exists. Thus Hooke, as before described, wished to ascertain whether or not there was any difference in the force of gravity at the top and bottom of St. Paul's Cathedral. The chemist continually performs analyses for the purpose of ascertaining whether or not a given element exists in a particular mineral or mixture; all such experiments and analyses are qualitative rather than quantitative, because though the result may be more or less, and is necessarily quantitative, the particular amount of the result is not the immediate object of the enquiry.

So soon, however, as a result is known to be discoverable, the scientific man ought to proceed to the strictly quantitative enquiry, how great a result follows from a certain amount of the conditions which are supposed to constitute the aggregate cause? The possible numbers of experiments are now indefinitely great, for every variation in a necessary condition will usually produce a variation in the amount of the effect. The method of variation which thus arises is no narrow or special method, but it is the general application of experiment to phenomena capable of continuous quantity. As Professor

Fowler has well remarked[a], the observation of variations is really an integration of a supposed infinite number of applications of the so-called method of difference, that is of experiment in its perfect form.

In induction we aim at establishing a general law, and if we deal with quantities that law must really be expressed more or less obviously in the form of an equation, or it may be in more than one equation. We treat as before of conditions, and of what happens under those conditions. But the conditions will now vary, not in quality, but quantity; and the effect will also vary in quantity, so that the result of quantitative induction is always to arrive at some mathematical expression involving the quantity of each condition, and expressing the quantity of the result. In other words, we wish to know what function the effect is of its conditions. We shall find that it is one thing to obtain the numerical results, and quite another thing to detect the law obeyed by those results, the latter being an operation of an inverse and tentative character.

The Variable and the Variant.

Almost every series of quantitative experiments is directed to obtain the relation between the different values of one quantity which is varied at will, and another quantity which is caused thereby to vary. We may conveniently distinguish these as respectively the *variable* and the *variant*. When we are examining the effect of heat in expanding bodies, heat, or one of its dimensions, temperature, is the variable, length the variant. If we compress a body to observe how much it is thereby heated, pressure, or it may be the dimensions of the body, forms the variable, heat the variant. In thermo-electric pile we make heat the variable and the

[a] 'Elements of Inductive Logic,' 1st edit. p. 175.

measure electricity as the variant. That one of the two measured quantities which is an antecedent condition of the other will be the variable.

It will always be convenient to have the variable entirely under our command. Experiments may indeed be made with accuracy, provided we can exactly measure the variable at the moment when the quantity of the effect is determined by it. But if we have to trust to the action of some capricious and very uncertain force, there may be great difficulty in making exact measurements, and those results may not be disposed over the whole range of quantity in a convenient manner. It is one prime object of the experimenter, therefore, to obtain a regular and governable supply of the cause or force which he is investigating. To determine correctly the efficiency of windmills, when the natural winds were constantly varying in force, would be exceedingly difficult. Smeaton, therefore, in his experiments on the subject, created a uniform artificial wind of the required force by moving his models against the air on the extremity of a revolving arm[b]. The velocity of the wind could thus be rendered greater or less, it could be maintained uniform for any length of time, and its amount could be exactly ascertained. In determining the laws of the chemical action of light it would be out of the question to employ the rays of the sun, which vary in intensity with the clearness of the atmosphere, and with every passing cloud. One great source of difficulty in photometry and the experimental investigation of the chemical action of light consists in obtaining a perfectly uniform and governable source of light rays[c].

[b] 'Philosophical Transactions,' vol. li. p. 138; abridgment, vol. xi. p. 355.

[c] See Bunsen and Roscoe's 'Researches,' in 'Philosophical Transactions' (1859), vol. cxlix. p. 880, &c., where they describe a constant flame of carbon monoxide gas.

Fizeau's method of measuring the velocity of light enabled him to appreciate the time occupied by light in travelling through a distance of eight or nine thousand metres. But the revolving mirror of Wheatstone subsequently enabled Foucault and Fizeau to measure the velocity in a space of four metres. In this latter method there was the obvious advantage that various media could be substituted for air, and the temperature, density, and other conditions of the experiment accurately governed or defined.

Measurement of the Variable.

There is little use in obtaining exact measurements of an effect unless we can also exactly measure the conditions with which the effect is to be connected. It is absurd to measure the electrical resistance of a piece of metal, its elasticity, tenacity, density, or other physical qualities, if these vary in degree, not only with the minute and almost inappreciable impurities of the metal, but also with its physical condition. If the same bar changes its properties by being heated and cooled, and we cannot exactly define the state in which it is at any moment, our care in measuring will be wasted, because it can lead to no law. It is of little use to determine very exactly the electric conductibility of carbon, which as graphite or gas carbon conducts like a metal, as diamond is almost a non-conductor, and in several other forms possesses variable and intermediate powers of conduction. It will be of use only for immediate practical applications. Before measuring these we ought to have something to measure of which the conditions are capable of exact definition, and to which at a future time we or others can recur. Similarly the accuracy of our measurement need not much surpass the accuracy with which we can define the conditions of the object treated.

The speed of electricity in passing through a conductor mainly depends upon the inductive capacity of the surrounding substances, and, except for technical or special purposes, there is little use in measuring velocities which in some cases are one hundred times as great as in other cases. But the maximum speed of electric conduction is probably a constant quantity of great scientific importance, and according to Prof. Clerk Maxwell's determination in 1868 is 174,800 miles per second, or little less than that of light. The true boiling point of water is a point on which all practical thermometry depends, and it is highly important to determine that point in relation to the absolute thermometric scale. But when water free from air and impurity is heated there seems to be no definite limit to the temperature it may reach, a temperature of 356° Fahr. having been actually observed. Such temperatures, therefore, do not require very accurate measurement. All meteorological measurements depending on the accidental condition of the sky are of infinitely less importance than physical measurements in which such accidental conditions do not intervene. Many profound investigations depend upon our knowledge of the radiant energy continually poured upon the earth by the sun; but this must be measured when the sky is perfectly clear, and the absorption of the atmosphere at its minimum. The slightest interference of cloud destroys the value of such a measurement, except for meteorological purposes, which are of vastly less generality and importance. It is seldom useful, again, to measure such a quantity as the height of a snow-covered mountain within a foot, when the thickness of the snow alone may cause it to vary 25 feet or more, when in short the height itself is indefinite to that extent[e].

[e] Humboldt's 'Cosmos' (Bohn), vol. i. p. 7.

Maintenance of Similar Conditions.

Our ultimate object in induction must be to obtain the complete relation between the conditions and the effect, but this relation will generally be so complex that we can only attack it in detail. We must, as far as possible, confine the variation to one condition at a time, and establish a separate relation between each condition and the effect. This will be at any rate the first step in approximating to the complete law, and it will be a subsequent question how far the simultaneous variation of several conditions modifies their separate actions. In many of the most important experiments, indeed, it is only one condition which we wish to study, and the others are merely interfering forces which we would gladly avoid if possible. One of the conditions of the motion of a pendulum is the resistance of the air, or other medium in which it swings; but when Newton was desirous of proving the equal gravitation of all substances, he had no interest in so entirely different a force as the effect of the air. His object was then to observe a single force only, and so it is in a great many other experiments. Accordingly one of the most important methods of investigation consists in maintaining all the conditions of like magnitude except that which is to be studied. As that admirable experimental philosopher, Gilbert, expressed it[f], 'There is always need of similar preparation, of similar figure, and of equal magnitude, for in dissimilar and unequal circumstances the experiment is doubtful.'

In Newton's decisive experiment similar conditions were provided for, with the usual simplicity which characterizes the highest art. The pendulums of which the oscillations were compared consisted of exactly equal boxes of wood, hanging by equal threads, and filled with different sub-

[f] Gilbert, 'De Magnete,' p. 109

stances, so that the total weights should be exactly equal and the centres of oscillation at the same distance from the points of suspension. Hence the resistance of the air became approximately a matter of indifference; for the outward size and shape of the pendulums being exactly the same, the absolute force of resistance would be the same, so long as the pendulums vibrated with equal velocity; and the weights being equal the force would diminish the velocity in like degree. Hence if any inequality were observed in the vibrations of the two pendulums, it must arise from the only circumstance which was different, namely the chemical character of the matter within the boxes. No inequality being observed, the chemical nature of substances can have no appreciable influence upon the force of gravitation[g].

A beautiful experiment was devised by Dr. Joule for the purpose of showing that the gain or loss of heat by a gas is connected, not with the mere change of its volume and density, but with the energy received or given out by the gas. Two strong vessels, connected by a tube and stop-cock, were surrounded entirely with water after the air had been exhausted from one vessel and condensed in the other to the extent of twenty atmospheres. The whole apparatus having been brought to a uniform temperature by agitating the water, and the temperature having been exactly observed, the stop-cock was opened, so that the air at once expanded and filled the two vessels uniformly. The temperature of the water being again noted was found to be almost entirely unchanged. The experiment was then repeated in an exactly similar manner, except that the strong vessels were placed in separate portions of water. It was then discovered that cold was produced in the vessel from which the air rushed, and an almost exactly equal quantity of heat appeared in that to which

[g] 'Principia,' bk. III. Prop. vi.

it was conducted. Thus Dr. Joule clearly proved that rarefaction produces as much heat as cold, and that only when there is a disappearance of mechanical energy will there be production of heat[h]. What we have to notice, however, is not so much the result of the experiment, as the admirably simple manner in which a single change in the apparatus, the separation of the portions of water surrounding the strong air vessels, is made to give indications of the utmost significance.

Collective Experiments.

There is an interesting class of experiments which enable us to observe an indefinite number of quantitative results in one act. Generally speaking, each experiment yields us but one number, and before we can approach the real processes of reasoning we must laboriously repeat measurement after measurement, until we can lay out a pretty complete curve of the variation of one quantity as depending on another. Now we can sometimes abbreviate this labour, by making one quantity vary in different parts of the same apparatus through every required amount. Thus in observing the height to which water rises by the capillary attraction of a glass vessel, we may take a series of glass tubes of different bore, and measure the height through which it rises in each. But if we take two glass plates, and place them vertically in water, so as to be in contact at one vertical side, and slightly separated at the other side, the interval between the plates varies through every intermediate width, and the water rises to a corresponding height, producing at its upper surface a hyperbolic curve.

The absorption of light in passing through a coloured liquid may be beautifully shown by enclosing the liquid

[h] 'Philosophical Magazine,' 3rd Series, vol. xxvi. p. 375.

in a wedge-shaped glass, so that we have at a single glance an infinite variety of thicknesses in view. As Newton himself remarked, a red liquid viewed in this manner is found to have a pale yellow colour at the thinnest part, and it passes through orange into red, which gradually becomes of a deeper and darker tint[i]. The effect may be noticed even in a common conical wine-glass. The prismatic analysis of light from such a wedge-shaped vessel discloses the reason, by exhibiting the progressive absorption of different rays of the spectrum as investigated by Dr. J. H. Gladstone[k].

A moving body may sometimes be made to mark out its own course, like a shooting star which leaves a tail behind it. Thus an inclined jet of water exhibits in the clearest manner the parabolic path of a projectile. In Wheatstone's Kaleidophone the curves produced by the combination of vibrations of different ratios are shown by placing bright reflective buttons on the tops of wires of various forms. The motions are performed so quickly that the eye receives the impression of the path as a complete whole, just as a burning stick whirled round produces a continuous circle. The laws of electric induction are beautifully shown when iron filings are brought under the influence of a magnet, and fall into curves corresponding to what Faraday called the Lines of Magnetic Force. When Faraday tried to define what he meant by his lines of force, he was obliged to refer to the filings. 'By magnetic curves,' he says[l], 'I mean lines of magnetic forces which would be depicted by iron filings.' Robison had previously produced similar curves by the action of frictional electricity[m], and from a mathematical investiga-

i 'Opticks,' 3rd edit. p. 159.

k Watts, 'Dictionary of Chemistry,' vol. iii. p. 637.

l 'Faraday's Life,' by Bence Jones, vol. ii. p. 5.

m Watts 'Dictionary of Chemistry,' vol. ii. pp. 402, 403.

tion of the forms of such curves we may infer that magnetic and electric attractions obey the general law of emanation, that of the inverse square of the distance. In the electric brush we have another similar exhibition of the laws of electric attraction.

There are several branches of science in which collective experiments have been used with great advantage. Lichtenberg's electric figures, produced by scattering electrified powder on an electrified resin cake, so as to show the condition of the latter, suggested to Chladni the notion of discovering the state of vibration of plates by strewing sand upon them. The sand collects at the points where the motion is least, and we gain at a glance a comprehension of the general form of undulation of the whole plate. To this method of experiment we owe the beautiful observations of Savart. The exquisite coloured figures exhibited by plates of crystal, when examined by polarized light, afford a more complicated example of the same kind of investigation. They led Brewster and Fresnel to a successful explanation of the properties of the optic axes of crystals. The unequal conduction of heat in crystalline substances has also been shown in a similar manner, by spreading a thin layer of wax over the plate of crystal, and applying heat to a single point. The wax then melts in a circular or elliptic area according as the rate of conduction is uniform or not. Nor should we forget that Newton's rings were an early and most important instance of investigations of the same kind, showing the effects of interference of light undulations of all magnitudes at a single view. Sir John Herschel gave to all such opportunities of observing directly the results of a general law, the name of *Collective Instances*[n], and I propose to adopt the name *Collective Experiments.*

[n] 'Preliminary Discourse,' &c, p. 185.

Such experiments will in many subjects only give the first hint of the nature of the law in question, but will not admit of any exact measurements. The parabolic form of a jet of water may well have suggested to Galileo his views concerning the path of a projectile; but it would not serve now for the exact investigation of the laws of gravity. It is not likely too that capillary attraction could be exactly measured by the use of inclined plates of glass, and the tubes would probably be better for precise investigation. As a general rule, these collective experiments would be most useful for popular instruction and illustration of the laws of science. But when the curves and figures produced are of a precise and permanent character, as in the coloured figures produced by crystalline plates, they may admit of exact measurement, and may often be the only mode of approaching the question. Newton's rings, diffraction fringes, and other effects of the interference of light, allow of very accurate measurements.

Under the class of collective experiments we may perhaps place those in which we render visible the motions of a mass of gas or liquid by diffusing some opaque substance in it. The behaviour of a body of air may often be studied in a beautiful way by the use of smoke, as in the production of smoke rings and jets. In the case of liquids lycopodium powder is sometimes employed. To detect the mixture of currents or strata of liquid, I employed exceedingly dilute solutions of common salt and silver nitrate, which produce a very visible cloud wherever they come into contact[o]. Atmospheric clouds often reveal to us the movements of great volumes of air which would otherwise be quite unapparent.

o 'Philosophical Magazine,' July, 1857, 4th Series, vol. xiv. p. 24.

Periodic Variations.

A very large and important class of investigations are concerned with Periodic Variations. We may define a periodic phenomenon as one which, with the constant and uniform change of the variable, returns time after time to the same value. If we strike a pendulum it presently returns to the point from which we disturbed it, and with the uniform progress of time goes on making excursions and returning, until stopped by the dissipation of its energy. If one body in space approaches by gravity towards another, they will revolve round each other in an elliptic orbit, and return for an indefinite number of times to the same relative positions. On the other hand a single body projected into empty space, away from the action of any extraneous force, would go on moving for ever in a straight line, according to the first law of motion. In the latter case the variation is called *secular*, because it proceeds during ages in a similar manner, and suffers no περίοδος or going round. It may be doubted whether there really is any motion in the universe which is not periodical. Mr. Herbert Spencer long since adopted the doctrine that all motion is ultimately rhythmical[p], and abundance of evidence may be adduced in favour of his view. The so-called secular acceleration of the moon's motion is certainly periodic, and as, so far as we can tell, no body is beyond the attractive power of other bodies, rectilinear motion becomes purely hypothetical, or at least infinitely improbable. All the motions of all the stars must tend to become periodic. Though certain disturbances in the planetary system seem to be uniformly progressive, Laplace is considered to have proved that they really have their limits, so that after an almost infinitely great time, all the planetary bodies might return to the

[p] 'First Principles,' 3rd edit. chap. x. p. 253.

same exact places, and the stability of the system be established.

But any such theory of periodic stability is really hypothetical, and does not take into account a multitude of phenomena resulting in the dissipation of energy, which may be a really secular process incapable of restoration. For our present purposes we really need not attempt to form any opinion on such lofty questions. Any change which does not present the appearance of a periodic character will be empirically regarded as a secular change for the present, so that there will be an abundant supply of non-periodic variations.

The variations which we produce experimentally will often be non-periodic. When we communicate heat to a gas it increases in bulk or pressure, and as far as we can go the higher the temperature the higher the pressure. Our experiments are of course restricted in temperature both above and below, but there is every reason to believe that the bulk being the same, the pressure would never return to the same point at any two different temperatures. We may of course repeatedly raise and lower the temperature at regular or irregular intervals entirely at our will, and the pressure of the gas will vary in like manner and exactly at the same intervals, but such an arbitrary series of changes would not constitute Periodic Variation. It would constitute a succession of distinct experiments, which would place beyond reasonable doubt the connexion of cause and effect.

Whenever a phenomenon recurs at equal or nearly equal intervals, there is, according to the theory of probability, considerable evidence of connexion, because if the recurrences were entirely casual it is exceedingly unlikely that they would happen at equal intervals. Thus the mere fact that a brilliant comet had appeared in the years 1301, 1378, 1456, 1531, 1607, and 1682, gave con-

siderable presumption in favour of the identity of the body apart from the similarity of the orbit. There is nothing which so strongly fascinates the attention of men as the recurrence time after time of some unusual event. Things and appearances which remain ever the same, like mountains and valleys, fail to excite the curiosity of a primitive people. It has been remarked by Laplace that even in his day the rising of Venus in its brightest phase never failed to excite surprise and interest. So there is little doubt that the first germ of physical science arose in the attention given by Eastern people to the changes of the moon and the motions of the planets. One of the earliest astronomical discoveries must have consisted in proving the identity of the morning and evening stars, on the ground of their similarity of aspect and invariable alternation[q]. Periodical changes of a somewhat complicated kind must have been understood by the Chaldæans, because they were aware of the cycle of 6585 days or 19 years which brings round the new and full moon upon the same days, hours, and even minutes of the year. The earliest efforts of scientific prophecy were founded upon this knowledge, and if at present we cannot help wondering at the precise anticipations of the nautical almanack, we may readily imagine the wonder excited by such successful predictions in early times.

Combined Periodic Changes.

We shall seldom or never find a body subject to a single periodic variation, and free from any other disturbances. As a general rule we may expect the periodic variation itself to undergo variation, which may possibly be secular or incapable of repetition, but is more likely to prove

[q] Laplace, 'System of the World,' vol. i. pp. 50, 54, &c.

periodic likewise; nor is there any limit to the complication of periods beyond periods, or periods within periods, which may ultimately be disclosed. In studying, then, a phenomenon of rhythmical character we have a succession of questions to ask. Is the periodic variation uniform? If not, is the change uniform? If not, is the change itself periodic? Is that new period uniform, or subject to any other change, or not? and so on *ad infinitum*.

In some cases there may be many distinct causes of periodic variations, and according to the principle of the superposition of small effects, to be afterwards more fully considered, these periodic effects will be simply added together, or at least approximately so, and the joint result may present a very complicated subject of investigation. Thus the tides of the ocean consist of a series of superimposed undulations, of which the number and character have by no means been determined as yet. Not only are there the ordinary and very obvious semi-diurnal tides caused by sun and moon, but a series of minor tides, such as the lunar diurnal, the solar diurnal, the lunar monthly, the lunar fortnightly, the solar annual and solar semi-annual are gradually being disentangled by the labours of Sir W. Thomson and others[r].

Variable stars present very interesting periodic phenomena; while some stars, δ Cephei for instance are subject to very regular and equal variations, others, like Mira Ceti, are less constant in the degrees of brilliancy which they attain or the rapidity of the changes, possibly on account of some much longer periodic variation[s]. The star β Lyræ presents a double maximum and minimum in each of its periods of nearly 13 days, and since the discovery of this variation the period in a period has probably been on the increase. 'At first the varia-

r 'British Association Report,' 1870, p. 120.

s Herschel's 'Outlines of Astronomy,' 4th edit. pp. 555–557.

bility was more rapid, then it became gradually slower; and this decrease in the length of time reached its limit between the years 1840 and 1844. During that time its period was nearly invariable; at present it is again decidedly on the decrease[t]." It is evident that the tracing out of such complicated variations presents an almost unlimited field for interesting investigation. The number of such variable stars already known is considerable, and there is no reason to suppose that any appreciable fraction of the whole number has yet been detected.

Principle of Forced Vibrations.

All investigations of the connection of periodic causes and effects rest upon a most important and general principle, which has been demonstrated by Sir John Herschel for some special cases, and clearly explained by him in several of his works[u]. The principle may be formally stated in the following manner: 'If one part of any system connected together either by material ties, or by the mutual attractions of its members, be continually maintained by any cause, whether inherent in the constitution of the system or external to it, in a state of regular periodic motion, that motion will be propagated throughout the whole systems, and will give rise, in every member of it, and in every part of each member, to periodic movements executed in equal period, with that to which they owe their origin, though not necessarily synchronous with them in their maxima and minima.' The meaning of the proposition is that the effect of a periodic cause will be periodic, and will recur at intervals equal to those of the

[t] Humboldt's 'Cosmos' (Bohn), vol. iii. p. 229.

[u] 'Encyclopædia Metropolitana,' art. *Sound*, § 323; 'Outlines of Astronomy,' 4th edit. § 650, pp. 410, 487–88; 'Meteorology,' Reprint, p. 137.

cause. Accordingly whenever we find any two phenomena which do proceed, time after time, through changes of exactly the same period, there is much probability that they are connected. It was in this manner, doubtless, that Pliny correctly conjectured that the cause of the tides lay in the sun and moon, the intervals between successive high tides being equal to the intervals between the moon's passage across the meridian. Kepler and Descartes too admitted the connection previous to Newton's demonstration of its precise nature. When Bradley discovered the apparent motion of the stars arising from the aberration of light, he was soon able to attribute it to the earth's annual motion, because it went through all its phases in exactly a year.

The most extensive and beautiful instance of induction concerning periodic changes which can be cited, is that of the discovery of an eleven-year period in various meteorological and astronomical phenomena. It would be difficult to mention any two things apparently more disconnected than the spots upon the sun and auroras. As long ago as 1826, Schwabe, of Dessau, commenced a regular series of observations of the spots upon the sun, which has been continued to the present time, and he was able to show that at intervals of about eleven years the spots increased much in size and number. Hardly was this discovery made known, than Dr. Lamont pointed out a nearly equal period of variation in the magnetic needle as regards declination. The occasional magnetic storms or sudden irregular disturbances of the needle were next shown to take place most frequently at the times when sun spots were prevalent, and as auroras are generally coincident with magnetic storms, these strange phenomena were brought into the cycle[x]. It has since been shown by

x 'Nature,' vol. i. p. 284; Quetelet, 'Sur la Physique du Globe,' pp. 148, 262-64, &c.

Professor Piazzi Smyth and Mr. E. J. Stone, that the temperature of the earth's surface as indicated by sunken thermometers gives some evidence of a like period. The existence of a periodic cause having once been established, it is quite to be expected, according to the principle of forced vibrations, that its influence will be more or less considerable in all meteorological phenomena.

Perhaps the most mysterious part of these investigations is that which refers the phenomena to the planetary configurations as an ulterior cause. Professor Balfour Stewart, with Messrs. Warren de la Rue and Loewy, by laborious researches discovered a periodic change of 584 days in the sun spots, coincident with changes in the relative positions of the Earth, Jupiter, and Venus. It has since been rendered probable by the researches of Dr. Kirkwood and others, that Schwabe's eleven-year period is due to the action of Mercury. Several other periods of more or less importance have been supposed to exist, but the subject is yet open to much more inquiry.

Integrated Variations.

In considering the infinite variety of modes in which one effect may depend upon another, we must set apart in a distinct class those which arise from the accumulated effects of a constantly acting cause. When water runs out of a cistern, the velocity of motion depends, according to Torricelli's theorem, on the height of the surface of the water above the vent; but the amount of water which leaves the cistern in a given time depends upon the aggregate result of that velocity, and is only to be ascertained by the mathematical process of integration. When one gravitating body falls towards another, the force of gravity varies according to the inverse square of the distance; to obtain the velocity produced we

must integrate or sum the effects of that law; and to obtain the space passed over by the body in any given time, we must again integrate with regard to the variable velocity.

In periodic variations the same distinction must be drawn. The heating power of the sun's rays at any place on the earth varies every day with the height attained, and is greatest about noon; but it does not follow that the temperature of the air is greatest at the same time. This temperature is an integrated effect of the sun's heating power, and as long as the sun is able to give more heat to the air than the air loses in any other way, the temperature continues to rise, so that the maximum is deferred until about 3 P.M. Similarly the hottest day of the year falls, on an average, about one month later than the summer solstice, and all the seasons lag about a month behind the motions of the sun. In the case of the tides, too, the effect of the sun's or moon's attractive power is never greatest when the power is greatest; the effect always lags more or less behind the cause. Yet the intervals between the successive tides are exactly equal, in the absence of disturbance, to the intervals between the passage of the sun or moon across the meridian. Thus the principle of forced vibrations holds true of all such cases.

In periodic phenomena, however, very curious results will sometimes follow from the integration of effects. If we strike a pendulum, and then repeat the stroke time after time when it is in the same part of the vibration, every stroke concurs with every other one in adding to the momentum, and we can thus increase the extent and violence of the vibrations to any degree. We can stop the pendulum again by strokes applied when it is moving in the opposite direction, and the successive effects being added together will soon bring it to rest. Now if we

alter the intervals of the strokes so that each two successive strokes act in opposite manners they will exactly neutralize each other, and the energy expended will be turned into heat or sound at the point of percussion. Exactly similar effects occur in all cases of rhythmical motion. If the musical note C is sounded in a room containing a piano, the string corresponding to it will be thrown into vibration, because every successive stroke of the air-waves upon the string finds it in like position as regards the vibration, and thus adds to its energy of motion. But the other strings being incapable of vibrating with the same rapidity are struck at various periods of their vibrations, and one stroke will sooner or later be opposed by one contrary in effect. All phenomena of *resonance* arise from this coincidence in time of undulation. The air in a pipe closed at one end, and about 12 inches in length, is capable of vibrating 512 times in a second. If, then, the note C is sounded in front of the open end of the pipe, every successive vibration of the air is treasured up as it were in the motion of the air. In a pipe of different length the pulses of air would strike each other, and the mechanical energy would be transmuted into heat and become no longer perceptible as sound.

These accumulated vibrations may sometimes become so intense as to lead to unexpected results. A glass vessel if touched with a violin bow at a suitable point may be fractured with the excess of vibration. In the same way a suspension bridge may readily be broken down if a company of soldiers walk across it in steps the intervals of which happen to agree with the intervals of vibration of the bridge itself. But if they break the step or march with very different time, they may have no perceptible effect upon the bridge. In fact if the impulses communicated to any vibrating body are exactly synchronous

with its vibrations, the energy of those vibrations will be unlimited, and may fracture any body.

Let us now consider what will happen if the strokes be not exactly at the same intervals as the vibrations of the body, but, say, a very little slower. Then a succession of strokes will meet the body in nearly but not quite the same position, and their effects will be accumulated. Afterwards the strokes will begin to fall when the body is in the opposite phase. Thus imagine that one pendulum moving exactly from one extreme point to another in a second, should be struck by another pendulum which makes 61 beats in a minute; then, if the pendulums commence together, they will at the end of $30\frac{1}{2}$ beats be moving in opposite directions. Hence whatever energy was communicated in the first half minute will be neutralized by the opposite effect of that given in the second half. The effect of the strokes of the second pendulum will therefore be alternately to increase and decrease the vibrations of the first, so that a new kind of vibration will be produced running through all its phases in 61 seconds. An effect of this kind was actually observed by Ellicott, a member of the Royal Society, in the case of two clocks[y]. He found that through the wood-work by which the clocks were connected a slight impulse was transmitted, and each pendulum alternately lost and gained momentum. Each clock, in fact, tended to stop the other at regular intervals, and in the intermediate times to be stopped by the other. Many of the most important disturbances in the planetary system depend upon the same principle; for if one planet happens always to pull another in the same direction in similar parts of their orbits, the effects, however slight, will be accumulated, and a disturbance of large ultimate amount and of long period will be produced. The

y 'Philosophical Transactions' (1739), vol. xli. p. 126.

long inequality in the motions of Jupiter and Saturn is thus due to the fact that five times the mean motion of Saturn is very nearly equal to twice the mean motion of Jupiter, causing a coincidence in their relative positions and disturbing powers[z].

[z] Grant's 'History of Physical Astronomy,' p. 59.

CHAPTER XXI.

THEORY OF APPROXIMATION.

In order that we may gain a true understanding of the kind, degree, and value of the knowledge which we acquire by experimental investigation, it is requisite that we should be fully conscious of its approximate character. We must learn to distinguish between what we can know and cannot know—between the questions which admit of solution, and those which only seem to be solved. Many persons may be misled by the expression *exact science*, and may think that the knowledge acquired by scientific methods admits of our reaching absolutely true laws, exact to the last degree. There is even a prevailing impression that when once mathematical formulæ have been successfully applied to a branch of science, this portion of knowledge assumes a new nature, and admits of reasoning of a higher character than those sciences which are still unmathematical.

The very satisfactory degree of accuracy attained in the science of astronomy gives a certain plausibility to erroneous notions of this kind. Some persons no doubt consider it to be *proved* that planets move in ellipses, in such a manner that all Kepler's laws hold exactly true; but there is a double error in any such notions. In the first place, Kepler's laws are *not proved*, if by proof we mean certain demonstration of their exact truth. In the next place, even assuming Kepler's laws to be exactly true in a

theoretical point of view, the planets never move according to those laws. Even if we could observe the motions of a planet, of a perfect globular form, free from all perturbing or retarding forces, we could never perfectly prove that it moved in an ellipse. To prove the elliptical form we should have to measure infinitely small angles, and infinitely small fractions of a second; we should have to perform impossibilities. All we can do is to show that the motion of an unperturbed planet approaches *very nearly* to the form of an ellipse, and the more nearly the more accurately our observations are made. But if we go on to assert that the path *is* an ellipse we pass beyond our data, and make an assumption which may be more or less probable, but cannot be proved, in the strict sense of that term.

But, secondly, as a matter of fact no planet does move in a perfect ellipse, or manifest the truth of Kepler's laws exactly. The very law of gravity prevents its own results from being clearly exhibited, because the mutual perturbations of the planets distort the elliptical paths. Those laws again hold exactly true only of infinitely small planetary bodies, and when two great globes, like the sun and Jupiter, attract each other, the law must be modified. The periodic time is then shortened in the ratio of the square root of the number expressing the sun's mass, to that of the sum of the numbers expressing the masses of the sun and planet, as was shown by Newton[a]. Even at the present day discrepancies exist between the observed dimensions of the planet's orbits and their theoretical magnitudes, after making allowance for all disturbing causes[b]. Nothing, in fact, is more certain in scientific method than that approximate coincidence can alone be expected. In the measurement of continuous quantity

[a] 'Principia,' bk. III. Prop. 15.

[b] See Lockyer's 'Lessons in Elementary Astronomy,' p. 301.

perfect correspondence must be purely accidental, and should give rise to suspicion rather than to satisfaction.

One remarkable result of the approximate character of our observations is that we never could prove the existence of perfectly circular or parabolic movement, even if it existed. The circle is a singular case of the ellipse, for which the eccentricity is zero; it is infinitely improbable than any planet, even if undisturbed by other bodies, should have a circle for its orbit; but if the orbit were a circle we could never prove the entire absence of eccentricity. All that we could do would be to declare the divergence from the circular form to be inappreciable. Delambre was unable to detect the slightest ellipticity in the orbit of Jupiter's first satellite, but he could only infer that the orbit was *nearly* circular. The parabola is the singular limit between the ellipse and the hyperbola. As there are elliptic and hyperbolic comets, so we might conceive the existence of a parabolic comet. Indeed if an undisturbed comet fell towards the sun from an infinite distance it would move in a parabola; but we could never prove that it so moved.

Substitution of Simple Hypotheses.

In truth men never can solve problems fulfilling the complex circumstances of nature. All laws and explanations are in a certain sense hypothetical, and apply exactly to nothing which we can know to exist. In place of the actual objects which we see and feel, the mathematician invariably substitutes imaginary objects, only partially resembling those represented, but so devised that the discrepancies may not be of an amount to alter seriously the character of the solution. When we probe the matter to the bottom physical astronomy is as hypothetical as Euclid's elements. There may exist in nature perfect

straight lines, triangles, circles, and other regular geometrical figures; to our science it is a matter of indifference whether they do or do not exist, because in any case they must be beyond our powers of appreciation. If we submitted a perfect circle to the most rigorous scrutiny and measurement, it is impossible that we should discover whether it were perfect or not. Nevertheless in geometry we argue concerning perfect rectilineal figures and curves, and the conclusions apply to existing objects so far as we can assure ourselves that they agree with the hypothetical conditions of our reasoning. Now this is in reality all that we can do in the most perfect of the sciences of nature.

Doubtless in astronomy we meet with the nearest approximation to actual conditions. The law of gravity is not a complex one in itself, and we believe it with much probability to be exactly true; but we cannot calculate out in any one case its accurate results. The law asserts that every particle of matter in the universe attracts every other particle, with a force depending on the masses of the particles and their distance. We cannot then know the force acting on any one particle unless we know the masses and distances and positions of all the other particles in the universe. The physical astronomer has from the first made a sweeping assumption, namely, that all the other millions of existing systems exert no perturbing effects in our planetary system, that is to say, no effects in the least appreciable. Thus the problem becomes at once hypothetical, because there is little doubt that gravitation between our sun and planets and other systems must exist in some degree. But even when they consider the relations of our planetary bodies *inter se*, all their processes are grossly approximative. In the first place they assume that each of the planets is a perfect ellipsoid, with a smooth surface and a homogeneous interior. That this assumption is untrue every mountain and valley, every

sea, every mine affords conclusive evidence. If the astronomer is to make his calculations perfect, he must not only take account of the Himalayas and the Andes, the Atlantic and Pacific, but the attraction of every hill, nay, every ant-hill, must be separately calculated, nor must the attractive power of any grain of sand be neglected. So far are they from having yet considered any local inequality of the surface, that they have not yet decided upon the general form of the earth; it is yet a matter of speculation whether or not the earth is an ellipsoid with three unequal axes[c]. If, as is probable, the globe is proved to be irregularly compressed in some directions, the calculations of astronomers will have to be repeated and refined, in order that they may approximate to the attractive power of such a body. If we cannot accurately learn the form of our own earth, how can we expect to ascertain that of the moon, the sun, and other planets, in some of which are probably irregularities of greater proportional amount.

The science of physical astronomy is yet in a further way merely approximative and hypothetical. Given perfectly homogeneous ellipsoids acting upon each other according to the law of gravity, the best mathematicians have never and perhaps never will determine exactly the resulting movements. Even when three bodies simultaneously attract each other the complication of effects is so great that only approximate calculations can be made. Astronomers have not even attempted the general problem of the simultaneous attractions of four, five, six, or more bodies, resolving the general problem into so many different problems of three bodies. The principle upon which the calculations of physical astronomy proceed, is to neglect every effect which could not lead to any quantity appreciable in observation, and the quantities rejected

c Thomson and Tait, 'Treatise on Natural Philosophy,' vol. i. p. 646.

are indefinitely more numerous and complex than the few larger terms which are retained. All then is merely approximate.

Concerning other branches of physical science the same general statements are even more evidently true. We speak and calculate about inflexible bars, inextensible lines, heavy points, homogeneous substances, uniform spheres, perfect fluids and gases, and we deduce an infinite number of beautiful theorems; but all is hypothetical. There is no such thing as an inflexible bar, an inextensible line, nor any one of the other perfect objects of mechanical science; they are to be classed with those other almost mythical existences, the straight line, triangle, circle, rectangle, &c., about which Euclid so freely discoursed. Take the simplest operation considered in statics—the use of a crowbar in raising a heavy stone, and we shall find, as Thomson and Tait have pointed out, that we neglect far more than we observe[d]. If we suppose the bar to be quite rigid, the fulcrum and stone perfectly hard, and the points of contact real points, we might give the true relation of the forces. But in reality the bar must bend, and the extension and compression of different parts involve us in difficulties. Even if the bar be homogeneous in all its parts, there is no mathematical theory capable of determining with accuracy all that goes on; if, as is infinitely more probable, the bar is not homogeneous, the complete solution will be indefinitely more complicated, but hardly more hopeless. No sooner had we determined the change of form according to simple mechanical principles, than we should discover the interference of thermodynamic principles. Compression produces heat and extension cold, and thus the conditions of the problem are modified throughout. In attempting a fourth approximation we should have to allow for the conduction

[d] 'Treatise on Natural Philosophy,' vol. i. pp. 337, &c.

of heat from one part of the bar to another. All these effects are utterly inappreciable in a practical point of view, if the bar be a good stout one; but in a theoretical point of view they entirely prevent our saying that we have solved a natural problem. The faculties of the human mind, even when aided by the wonderful powers of abbreviation conferred by analytical methods, are utterly unable to cope with the complications of any one real problem. And had we exhausted all the known phenomena of a mechanical problem, how can we tell that hidden phenomena, as yet undetected, do not intervene in the commonest actions. It is plain that no phenomenon comes within the sphere of our senses unless it possesses a certain momentum or magnitude capable of irritating the appropriate nerves. There may then, and, in fact, must be indefinite worlds of phenomena too slight to rise within the scope of our consciousness.

All the instruments with which we perform our measurements are fallible and faulty. We assume that a plumb-line gives a perfectly vertical line; but this is never true in an absolute sense, owing to the attraction of mountains and other inequalities in the surface of the earth. In an accurate trigonometrical survey, the divergencies of the plumb-line must be approximately determined and allowed for[e]. We assume a surface of mercury to be perfectly plane, but even in the breadth of 5 inches there is a calculable divergence from a true plane of about one ten-millionth part of an inch; and this surface further diverges from true horizontality as the plumb-line does from true verticality. That most perfect instrument, the pendulum, is not even theoretically perfect, except for infinitely small arcs, and the delicate experiments performed with the torsion balance proceed on the assumption that the force of torsion of a wire is proportional to the angle of

e Pratt, 'Philosophical Transactions,' vol. cxlvi. p. 31.

torsion, which is again only true for infinitely small angles[f].

We need to take great care that in simplifying a problem we do not overlook some circumstance which from peculiar mathematical conditions is of importance. Thus in experiments upon the density of the earth we may treat irregularities of its contour as producing inconsiderable effects. But a like assumption must not be made concerning irregularities in the strata of the earth at a short distance below the point of experiment[g].

Such is the purely approximate character of all our operations that it is not uncommon to find the theoretically worse method giving truer results than the theoretically perfect method. The common pendulum which is not isochronous is better for practical purposes than the cycloidal pendulum which is isochronous in theory, but subject to mechanical difficulties. The spherical form is not the correct form for a speculum or lense, but it differs so slightly from the true form, and is so much more easily produced mechanically, that it is generally best to rest content with the spherical surface. Even in a six-feet mirror the difference between the parabola and the sphere is only about $\frac{1}{10,000}$ of an inch, a thickness which would be taken off in a few rubs of the polisher. Watts' ingenious parallel motion was intended to produce rectilinear movement of the piston rod. In reality the motion was always curvilinear, but a certain part of the curve approximated sufficiently for his purposes to a straight line.

Approximation to Exact Laws.

Though we can never prove any numerical law with perfect accuracy, it would be a great mistake to suppose

[f] Baily, 'Memoirs of the Royal Astronomical Society,' vol. xiv, p. 99.

[g] Airy, Philosophical Transactions,' vol. cxlvi, p. 334.

that there is any inexactness in the laws of nature. We may even discover a law which we believe to represent the action of forces with perfect exactness. The mind may seem to pass in advance of its data, and choose out certain numerical results as absolutely true. We can never really pass beyond our data, and so far as assumption enters in, so far want of certainty will attach to our conclusions; nevertheless we may in many cases rightly prefer a probable assumption of a precise law to numerical results, which are at the best only approximative. We must accordingly draw a strong distinction between the laws of nature which we believe to be accurately stated in our formulas, and those to which our statements only make an approximation, so that at a future time the law will be differently stated.

The law of gravitation is expressed in the form $F = \frac{Mm}{D^2}$, meaning that gravity is proportional directly to the product of the gravitating masses, and indirectly to the square of their distance. The latent heat of steam, again, is expressed by the equation $\log F = \alpha + b\alpha^t + c\beta^t$, in which are five quantities a, b, c, α, β, to be determined by experiment. Now there is every reason to believe that in the progress of science the law of gravity will remain entirely unaltered, and the only effect of further inquiry will be to render it a more and more probable expression of the absolute truth. The law of the latent heat of steam, on the other hand, will be modified by every new series of experiments, and it may not improbably be shown that the assumed law can never be made to agree with the results of experiment.

Philosophers have by no means always supposed that the law of gravity was exactly true. Newton, though he had the highest confidence in its truth, admitted that there were motions in the planetary system which he

could not reconcile with the law. Euler and Clairaut who were, with D'Alembert, the first to apply the full powers of mathematical analysis to the theory of gravitation as explaining the perturbations of the planets, did not treat the law as sufficiently established to attribute all discrepancies to the errors of calculation and observation. In short, they did not feel certain that the force of gravity exactly obeyed the well known rule. The law might have involved other powers of the distance. It might have been expressed, for example, in the form

$$F = \cdots + \frac{a}{D} + \frac{b}{D^2} + \frac{c}{D^3} + \cdots$$

and the coefficients a and c might have been so small that those terms would only become apparent in very accurate comparisons with fact. Attempts have been made from time to time to account for difficulties, by attributing value to such neglected terms. Gauss at one time thought that the even more fundamental principle of gravity, that the force is dependent only on mass and distance, might not be exactly true, and he undertook accurate pendulum experiments to test this opinion. Only as these repeated doubts have been time after time resolved in favour of the law of Newton, has it been assumed as precisely correct. But this belief does not rest on experiment or observation only. The calculations of physical astronomy, however accurate, could never show that the other terms of the above general expression were absolutely devoid of value. It could only be shown that they had such slight value as never to become apparent.

There are, however, other theoretical reasons why the law is probably complete and true as commonly stated. Whatever influence or power spreads from a point, and expands uniformly through space, will doubtless vary inversely in intensity as the square of the distance, simply because the area over which it is spread increases as the

square of the radius. This part of the law of gravity may be considered as due to the properties of space, and there is a perfect analogy in this respect between gravity and all other *emanating* forces or substances, as was pointed out in a most comprehensive and clear manner by Keill [h]. Thus the undulations of light, heat, sound, and the attractions of electricity or magnetism obey the very same law so far as we can ascertain. If the molecules of a gas or the particles of matter constituting odour were to start from a point and move from it in straight lines uniformly, their distances would increase and their density decrease according to the same principles.

The other known laws of nature stand in a precisely similar position. Dalton's laws of definite combining proportions never have been, and never can be exactly proved; but chemists having shown, to a considerable degree of approximation, that all the more common elements combine together as if each element had atoms of an invariable mass, assume that this is exactly true. They go even further. Prout pointed out in 1815 that the equivalent weights of the elements appeared to be simple commensurable numbers; and Dumas, Pelouze, Marignac, Erdmann, Stas, and others have gradually rendered it likely that the atomic weights of hydrogen, carbon, oxygen, nitrogen, chlorine, and silver, are in the ratios of the numbers 1, 12, 16, 14, 35·5, and 108. Chemists then step beyond their data; they throw aside their actual experimental numbers, and assume that the true ratios are not those exactly indicated by any weighings, but the simple ratios of these numbers. They boldly assume that the discrepancies are due to experimental errors, and they are justified by the fact that the more elaborate and skilful the researches on the subject, the more nearly their assumption is verified.

h 'An Introduction to Natural Philosophy,' 3rd. edit., 1733, p. 5.

Potassium is the only element whose atomic weight has been determined with great care, but which has not shown an approach to a simple ratio with the other elements. This exception may be due to some unsuspected cause of error[i]. A similar assumption is also made in the law of definite combining volumes of gases, and Sir B. C. Brodie has clearly pointed out the line of argument by which the chemist, observing that the discrepancies between the law and fact are within the limits of experimental error, assumes that they are due to error[k].

Faraday, in one of his researches, expressly makes an assumption of the same kind. Having shown, with some degree of experimental precision, that there exists a simple proportion between quantities of electrical energy and the quantities of chemical substances which it can decompose, so that for every atom dissolved in the battery cell an atom ought theoretically, that is without regard to dissipation of some of the energy, to be decomposed in the electrolytic cell, he does not stop at his numerical results. 'I have not hesitated,' he says, 'to apply the more strict results of chemical analysis to correct the numbers obtained as electrolytic results. This, it is evident, may be done in a great number of cases[l], without using too much liberty towards the due severity of scientific research.'

The law of the conservation of energy itself, one of the widest of all physical generalizations, must rest upon the same footing. The most that we can do by experiment is to show that the energy entering into any experimental combination is almost exactly equal to what comes out of it, and more nearly so the more accurately we perform all the measurements. Absolute equality is always a matter of assumption. We cannot even prove the indestructibility

i Watts, 'Dictionary of Chemistry,' vol. i. p. 455.

k 'Philosophical Transactions,' (1866) vol. clvi. p. 809.

l 'Experimental Researches in Electricity,' vol. i. p. 246.

of matter; for were an exceedingly minute fraction of existing matter to vanish in any experiment, say one part in ten millions, we could never detect the loss.

Successive Approximations to Natural Conditions.

When we examine the history of scientific problems, we find that one man or one generation is usually able to make but a single step at a time. A problem is always solved for the first time by making some bold hypothetical simplification, upon which the next investigator makes hypothetical modifications approaching more nearly to the truth. Errors are successively pointed out in previous solutions, until at last there might seem little more to be desired. Careful examination, however, will show that an indefinite series of minor inaccuracies remain to be corrected and explained, were our powers of reasoning sufficiently great, and the purpose adequate in importance.

Newton's successful solution of the problem of the planetary movements entirely depended at first upon a great but hypothetical simplification. The law of gravity only applies directly to two infinitely small particles, so that when we deal with vast globes like the earth, Jupiter, or the sun, we have an immense aggregate of separate attractions to deal with, and the law of the aggregate need not coincide with the law of the elementary particles. But Newton, by a great effort of mathematical reasoning, was able to show that two homogeneous spheres of matter act as if the whole of their masses were concentrated at the centres; in short, that such spheres are aggregates which manifest the simple law of gravity or are centrobaric bodies (vol. i. p. 423). He was then able with comparative ease to calculate the motions of the planets on the hypothesis of their being spheres, and to show that the results roughly agreed with observation.

Newton, indeed, was one of the few men who could make two great steps at once. He did not rest contented with the spherical hypothesis; having reason to believe that the earth was really a spheroid with a protuberance around the equator, he proceeded to a second approximation, and proved that the attraction of the protuberant matter upon the moon accounted for the precession of the equinoxes, and led to various complicated effects. But, as I have already mentioned (vol. ii. p. 76), even the spheroidal hypothesis is far from the truth. It takes no account of the irregularities of surface, the great protuberance of land, for instance, in Central Asia and South America, and the deficiency in the bed of the Atlantic.

To determine the law according to which a projectile, such as a cannon ball, moves through the resisting atmosphere is a problem very imperfectly solved at the present day, but in which many successive advances have been made. So little was known concerning the subject three or four centuries ago that a cannon ball was supposed to move at first in a straight line, and only after a time to be deflected into a curve. Tartaglia ventured to maintain that the path was curved throughout, as by the principle of continuity it should be; but the ingenuity of Galileo was required to prove this opinion, and to show that the curve was approximately a parabola. It is only, however, under several forced hypotheses that we can assert the path of a projectile to be truly a parabola: the path must be through a perfect vacuum, where there is no resisting medium of any kind; the force of gravity must be equal and act in parallel lines; and the moving body must be either a mere point, or a perfect centrobaric body, that is a body possessing a definite centre of gravity. None of these conditions can be really fulfilled in practice. The next great step in the problem was made by Newton and Huyghens, the latter of whom asserted that the atmo-

sphere would offer a resistance proportional to the velocity of the moving body, and concluded that the path would have in consequence a logarithmic character. Newton investigated in a general manner the subject of resisting media, and came to the conclusion that the resistance was more nearly proportional to the square of the velocity. The subject then fell into the hands of Daniel Bernouilli, who pointed out the enormous resistance of the air in cases of rapid movement, and calculated that a cannon ball, if fired vertically in a vacuum, would rise eight times as high as in the atmosphere. In more recent times an immense amount both of theoretical and experimental investigation has been spent upon the subject, since it is one of great importance in the art of war. Successive approximations to the true law have been made, but nothing like a complete and final solution has been achieved or even hoped for[m].

It is quite to be expected that the earliest experimenters in any branch of science will overlook corrections which afterwards become most apparent. The Arabian astronomers determined the meridian by taking the middle point between the places of the sun when at equal altitudes on the same day. They overlooked the fact that the sun has its own motion among the stars in the time intervening between the observations. Newton thought that the mutual disturbances of the planets might be disregarded, excepting perhaps the effect of the mutual attraction of the greater planets, Jupiter and Saturn, near their conjunction[n]. The expansion of quicksilver was long used as the measure of temperature, in ignorance or disregard of the fact that the rate of expansion increases with the temperature. Rumford, in the first experiment leading to a determination of the mechanical equivalent of

[m] Hutton's 'Mathematical Dictionary,' vol. ii. pp. 287–292.

[n] 'Principia,' bk. iii. Prop. 13.

heat, disregarded the heat absorbed by the box containing the water heated and by other parts of the apparatus, otherwise he would in Dr. Joule's opinion, have come nearly to the correct result.

It is surprising to learn the number of causes of error which enter into even the simplest experiment, when we strive to attain the most rigid accuracy. Thus we cannot perform the simple experiment of compressing a portion of gas in a bent tube by a column of mercury, in order to test the truth of Boyle's Law, without paying regard to,—(1) the variations of atmospheric pressure, which are communicated to the gas through the mercury; (2) the compressibility of mercury, which causes the column of mercury to vary in density; (3) the temperature of the mercury throughout the column; (4) the temperature of the gas which is with difficulty maintained invariable; (5) the expansion of the glass tube containing the gas. Although Regnault took all these circumstances into account in his accurate examination of the law [o], there is no reason for supposing that he exhausted the sources of inaccuracy.

All the earlier investigations concerning the nature of waves in elastic media proceeded upon the assumption that waves of different length would travel with equal speed. Newton's theory of sound had led him to this conclusion, and experiment, or indeed the commonest observations (see vol. i. p. 344) had sufficiently verified the inference. When the undulatory theory came to be applied at the commencement of this century to explain the phenomena of light, a great difficulty was encountered. The angle at which a ray of light is refracted in entering a denser medium depends, according to that theory, on the velocity with which the wave travels, so that if all waves of light were to travel with equal velocity in the same

[o] Jamin, 'Cours de Physique,' vol. i. pp. 282–3.

medium, the dispersion of mixed light by the prism and the production of the spectrum could not take place. Some of the most striking phenomena were thus in direct conflict with the theory. The great French mathematician, Cauchy, first pointed out the true explanation, namely that all previous investigators had made an arbitrary assumption for the sake of simplifying the calculations. They had assumed that the particles of the vibrating medium are so close together that the intervals are quite inconsiderable compared with the length of the wave, or in other terms infinitely small. This hypothesis happened to be approximately true in the case of air, so that no error was discovered in experiments on sound. Had it not been so, the earlier analysts would probably have failed to give any solution, and the progress of the subject might have been retarded. Cauchy was able to make a new approximation to truth under the more difficult supposition, that the particles of the vibrating medium are situated at considerable distances, and act and react upon the neighbouring particles by attractive and repulsive forces. To calculate the rate of propagation of a disturbance in such a medium is a work of excessive difficulty. The complete solution of the problem appears indeed to be beyond human power, so that we must be content, as in the case of the planetary motions, to look forward to successive approximations. All that Cauchy could do was to show that certain mathematical terms or quantities, neglected in previous theories, became of considerable amount under the new conditions of the problem, so that there will exist a relation between the length of the wave, and the velocity at which it travels. To remove, then, the difficulties in the way of the undulatory theory of light, a new approach to probable conditions was needed [p].

[p] Lloyd's 'Lectures on the Wave Theory,' pp. 22, 23.

In a similar manner Fourier's theory of the conduction and radiation of heat was based upon the hypothesis that the quantity of heat passing along any line is simply proportional to the rate of change of temperature. But it has since been shown by Forbes that the conductivity of a body diminishes as its temperature increases. All the details of Fourier's solution therefore require modification, and the results are in the meantime to be regarded as only approximately true [q].

We ought to distinguish between those problems which are physically and those which are merely mathematically incomplete. In the latter case the physical law is correctly seized, but the mathematician neglects, or is more often unable to follow out the law in all its results. The law of gravitation and the principles of harmonic or undulatory movement, even supposing the data to be correct, can never be followed into all their ultimate results. Dr. Young explained the production of Newton's rings by supposing that the rays reflected from the upper and lower surfaces of a thin film of a certain thickness were in opposite phases, and thus neutralized each other. It was pointed out, however, that as the light reflected from the nearer surface must be undoubtedly a little brighter than that from the further surface, the two rays ought not to neutralize each other so completely as they are observed to do. It was finally shown by Poisson that the discrepancy arose only from incomplete solution of the problem; for the light which has once got into the film must be to a certain extent reflected backwards and forwards *ad infinitum;* and if we follow out this course of the light by a perfect mathematical analysis, absolute darkness may be shown to result from the interference of the rays [r]. In such a case as this we used no physical laws

[q] Tait's 'Thermodynamics,' p. 10.

[r] Lloyd's 'Lectures on the Wave Theory,' pp. 82, 83.

but those of reflection and refraction, and the only difficulty consisted in developing their full consequences.

There is one instructive result of the theory of error which should always be borne in mind, namely that when a large variable error is combined with a small variable error, the uncertainty of the final result, as measured by its probable error, is scarcely at all affected by the small variable error[8]. Accordingly our efforts at accuracy must be devoted to the sources of error in the order of their magnitude. There is no use in making instruments to measure the heat of the sun with the last degree of accuracy, when the varying transparency of the atmosphere produces uncertainties of far greater amount. It is needless to observe a comet or other heavenly body with the very finest instruments if it appears low down on the horizon, where the atmospheric refraction is not accurately determinate. In short, minuter *variable* sources of error may be entirely neglected, so long as those of a considerably greater amount remain beyond our powers of correction.

Discovery of Hypothetically Simple Laws.

In some branches of science we meet with natural laws of a simple character which are in a certain point of view exactly true and yet can never be manifested as exactly true in natural phenomena. Such, for instance, are the laws concerning what is called a *perfect gas*. The gaseous state of matter is that in which the general properties of matter are exhibited in the simplest and most general manner. There is much advantage accordingly in approaching the question of molecular mechanics from this side. But when we ask the question—What is a gas? the answer must be a hypothetical one. Finding that

[8] Airy, 'Philosophical Transactions,' (1856) vol. cxlvi. p. 324.

gases *nearly* obey the law of Boyle and Marriotte; that they *nearly* expand by heat at the uniform rate of one part in 272·9 of their volume at 0° for each degree centigrade; and that they *more nearly* fulfil these conditions the more distant the point of temperature at which we examine them from the liquefying point, we pass by the principle of continuity to the conception of a perfect gas. Such a gas would probably consist of atoms of matter at so great a distance from each other as to exert no attractive forces upon each other; but for this condition to be exactly fulfilled the distances must be infinite, so that an absolutely perfect gas cannot exist. But the perfect gas is not merely a limit to which we may approach, it is a limit passed by at least one real gas. It has been shown by Despretz, Pouillet, Dulong, Arago, and finally Regnault, that all gases diverge from the Boylean law, and in nearly all cases the density of the gas increases in a somewhat greater ratio than the pressure, indicating a tendency on the part of the molecules to approximate of their own accord, and condense into liquid. In the more condensible gases such as sulphurous acid, ammonia, and cyanogen, this tendency is strongly apparent near the liquefying point. Hydrogen on the contrary diverges from the law of a perfect gas in the opposite direction, that is, the density increases less than in the ratio of the pressure[t]. This is a singular exception, the bearing of which I am unable to comprehend.

All gases diverge again from the law of uniform expansion by heat, but the divergence is less as the gas in question is less condensible, or examined at a temperature more removed from its liquefying point. Thus the perfect gas in this respect must have an infinitely high temperature. According to Dalton's law each gas in a mixture retains its own properties wholly unaffected by the presence

[t] Jamin, 'Cours de Physique,' vol. i. pp. 283–288.

of any other gas[u]. This law is probably true only by approximation, but it is obvious that it would be true of the perfect gas with infinitely distant particles[x].

Mathematical Principles of Approximation.

The whole subject of the approximate character of physical science will be rendered more plain if we consider it from a general mathematical point of view. Throughout quantitative investigations we deal with the relation of one quantity to certain other quantities, of which it is a function; but the subject is quite sufficiently complicated if we view one quantity as a function of one other. Now, as a general rule, a function can be developed or expressed as the sum of certain other quantities, the values of which depend upon the successive powers of the variable quantity. Thus, if y be the one quantity which is regarded as a function of x, then we may say that

$$y = A + Bx + Cx^2 + Dx^3 + Ex^4 + \ldots.$$

In this equation, A, B, C, D, &c., are fixed quantities, of different values in different cases. The terms may be infinite in number or after a time may cease to have any value. Any of the co-efficients A, B, C, &c., may be zero or negative; but whatever they may be they are fixed. The quantity x on the other hand may be made what we like, being variable at our will. Suppose, in the first place, that x and y are both measurable lengths. Let us assume that $\frac{1}{10.000}$ part of an inch is the least that we can take note of. Then when x is one hundredth of an inch, we have $x^2 = \frac{1}{10.000}$, and if C be less than unity, the term Cx^2 will be inappreciable, being less than we

[u] Joule and Thomson, 'Philosophical Transactions,' 1854, vol. cxliv. p. 337.

[x] The properties of a perfect gas have been described by Rankine, 'Transactions of the Royal Society of Edinburgh,' vol. xxv. p. 561.

can measure. Unless any of the quantities D, E, &c., should happen to be very great, it is evident that all the succeeding terms will also be inappreciable, because the powers of x become rapidly smaller in geometrical ratio. Thus when x is made small enough the quantity y seems to obey the equation

$$y = A + Bx.$$

If x should be made still less, if it should become so small, for instance, as $\frac{1}{1.000.000}$ of an inch, and B should not be very great, then y would appear to be the fixed quantity A, and would not seem to vary with x at all. On the other hand, were x to grow greater, say equal to $\frac{1}{10}$ inch, and C not be very small, the term Cx^2 would become appreciable, and the law would now be more complicated.

We can invert the mode of viewing this question, and suppose that while the quantity y undergoes variations depending on many powers of x, that our power of detecting the changes of value is more or less acute. While our powers of observation remain very rude and imperfect we may even be unable to detect any change in the quantity at all, that is to say Bx may always be smaller than to come within our notice, just as in former days the fixed stars were so called because they remained at apparently fixed distances from each other. With the use of telescopes and micrometers we become able to detect the existence of some motion, so that the distance of one star from another may be expressed by $A + Bx$, the term including x^2 being still inappreciable. Under these circumstances the star will seem to move uniformly, or in simple proportion to the time, x. With much improved means of measurement it will probably be found that this uniformity of motion is only apparent, and that there exists some acceleration or retardation due to the next term. More and more careful investigation will show

the law to be more and more complicated than was previously supposed.

There is yet another way of explaining the apparent results of a complicated law. If we take any curve and regard only a portion of it free from any kind of discontinuity, we may represent the character of such portion by an equation of the form

$$y = A + B x + C x^2 + D x^3 + \ldots\ldots$$

Restrict the attention to a very small portion of the curve, and the eye will be unable to distinguish its difference from a straight line, which amounts to saying that in the portion examined the term $C x^2$ has no value appreciable by the eye. Take a larger portion of the curve and it will be apparent that it possesses curvature, but it will be possible to draw a parabola or ellipse so that the curve shall be apparently coincident with a portion of that parabola or ellipse. In the same way if we take larger and larger arcs of the curve it will assume the character successively of a curve of the third and fourth degrees; that is to say, it corresponds to equations involving the third and fourth powers of the variable quantity.

We have arrived then at the conclusion that every phenomenon, when its amount can only be rudely measured, will either be of fixed amount, or will seem to vary uniformly like the distance between two inclined straight lines. More exact measurement may show the error of this first assumption, and the variation will then appear to be like that of the distance between a straight line and a parabola or ellipse. We may afterwards find that a curve of the third or higher degrees is really required to represent the variation. I propose to call the variation of a quantity *linear*, *elliptic*, *cubic*, *quartic*, *quintic*, &c., according as it is discovered to involve the first, second, third, fourth, fifth or higher powers of the variable. It is a general rule in quantitative investigation that we com-

mence by discovering linear, and afterwards proceed to elliptic or more complicated laws of variation. The approximate curves which we employ are all, according to De Morgan's use of the name, parabolas of some order or other; and since the common parabola of the second order is approximately the same as a very elongated ellipse, and is in fact an infinitely elongated ellipse, it is convenient and proper to call variation of the second order *elliptic*. It might also be called *quadric* variation.

As regards many important phenomena we are yet only in the first stage of approximation. We know that the sun and many so-called fixed stars, especially 61 Cygni, have a proper motion through space, and the direction of this motion at the present time is known with some degree of accuracy. But it is hardly consistent with the theory of gravity that the path of any body should really be a straight line. Hence, we must regard a rectilinear path as only an approximate and provisional description of the motion, and look forward to the time when its curvature will be ultimately detected and measured, though centuries perhaps must first elapse.

On the surface of the earth we are accustomed to assume that the force of gravity is uniform at all ordinary heights above or below the surface, because the variation is of so slight an amount that we are scarcely able to detect it. But supposing we could measure the variation, we should find it simply proportional to the height. Taking the earth's radius to be unity, let h be the height at which we measure the force of gravity. Then by the well-known law of the inverse square, that force will be proportional to

$$\frac{g}{(1+h)^2}, \text{ or to } g\,(1 - 2h + 3h^2 - 4h^3 + \ldots\ldots).$$

But at all heights to which we can attain h will be so small a fraction of the earth's radius that $3h^2$ will be in-

appreciable, and the force of gravity will seem to follow the law of linear variation, being proportional to $1 - 2h$.

When the circumstances of an experiment are much altered, different powers of the variable may become prominent. The resistance of a liquid to a body moving through it may be approximately expressed as the sum of two terms respectively involving the first and second powers of the velocity. At very low velocities the first power is of most importance, and the resistance, as Professor Stokes has shown, is nearly in simple proportion to the velocity. When the motion is rapid the resistance increases in a still greater degree, and is more nearly proportional to the square of the velocity.

Approximate Independence of Small Effects.

One result of the general theory of approximation possesses such great importance in physical science, and is so often applied, that we may consider it separately. The investigation of causes and effects is immensely simplified when we may consider each cause as producing its own effect invariably, whether other causes are acting or not. Thus, if the body P produces the effect x, and Q produces y, the question is whether P and Q acting together will produce simply the sum of the separate effects, $x + y$. It is under this supposition that we treated the methods of eliminating error (Chap. XV.), and errors of a less amount would still remain if the supposition was a forced and unnatural one. There are probably some parts of science in which the supposition of independence of effects holds rigidly true. The mutual gravity of two bodies, for instance, is entirely unaffected by the presence or absence of other gravitating bodies. People do not usually consider that this important principle is involved in such a simple thing as putting two pound weights in

the scale of a balance. How do we know that two pound weights together will weigh twice as much as one? Do we know it to be exactly so? Like other results founded on induction we cannot prove it certainly and absolutely, but all the calculations of physical astronomy proceed upon the assumption, so that we may consider it proved to a very high degree of approximation. We may, in fact, assume with much probability that bodies gravitate in entire independence of each other. Had not this been true the calculations of physical astronomy would have been almost infinitely more complex than they actually are, and the progress of knowledge would have been vastly slower.

The science of the spectrum again is much simplified by the fact that elements do not apparently interfere with each other in the production of light. The spectrum of sodium chloride is the spectrum of sodium superposed upon that of chlorine. Were it otherwise, we should have as many distinct spectra as there are distinct compounds in chemistry, and the subject would be almost hopelessly complex. The spectrum of a substance would then no more enable us to tell its components than the appearance of a new mineral indicates its composition. But it would probably be too early to assert the entire absence of any joint spectra. There is so much yet unexplained in the subject that some effects due to the mutual action of elements may possibly be discovered, and the independence will then be only approximate.

It is a general principle of scientific method that if effects be of small amount, comparatively to our means of observation, all joint effects will be of a higher order of smallness, and may therefore be rejected in a first approximation. This principle was distinctly employed by Daniel Bernouilli in the theory of sound, under the title of 'The Principle of the Coexistence of Small Vibrations.'

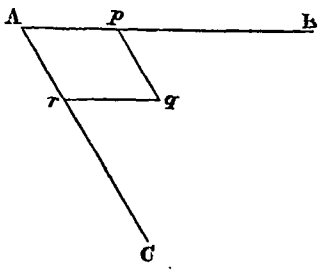
He showed that if a string is affected by two kinds of vibrations, we may consider each to be going on as if the other did not exist. We cannot perceive that the sounding of one musical instrument prevents or even modifies the sound of another, so that all sounds would seem to travel through the air, and act upon the ear in independence of each other. An exactly similar assumption is made in the theory of tides, which are really great waves. One wave is produced by the attraction of the moon, and another by the attraction of the sun, and the question arises, whether when these waves coincide, as at the time of spring tides, the joint wave will be simply the sum of the separate waves. On the principle of Bernouilli this will be so, because the tides on the ocean are almost indefinitely small compared with the depth of the ocean.

The principle of Bernouilli, however, is only approximately true. A wave never is exactly the same when another wave is interfering with it, but the less the displacement of particles due to each wave, the less in a still higher degree is the effect of one wave upon the other. In recent years Helmholtz was led to suspect that some of the phenomena of sound might after all be due to resultant effects overlooked by the assumption of previous physicists. He investigated the secondary waves which would arise from the interference of considerable disturbances, and was able to show that certain summation or resultant tones ought to be heard, and experiments subsequently devised for the purpose showed that they might be heard.

Throughout the mechanical sciences the *Principle of the Superposition of Small Motions* is of fundamental importance[y], and it may be thus explained. Suppose that two forces, acting from the points B and C, are simultaneously moving a body A. Let the force acting

[y] See Thomson and Tait's 'Natural Philosophy,' vol. i. p. 60.

from B be such that in one second it would move A to p, and similarly let the second force, acting alone, move A to r. The question arises, then, whether their joint action will urge A to q along the diagonal of the parallelogram. May we say that A will move the distance Ap in the direction AB, and Ar in the direction AC, or, what is the same thing, along the parallel line pq? In all strictness we cannot say so; for when A has moved towards p, the force from C will no longer act along the line AC, and similarly the motion of A towards r will modify the action of the force from B. This interference of one force with the line of action of the other will evidently be greater the larger is the extent of motion considered; on the other hand, as we reduce the parallelogram Apqr, compared with the distances AB and AC, the less will be the interference of the forces. Accordingly mathematicians avoid all error by considering the motions as infinitely small, so that the interference becomes of a still higher order of infinite smallness, and may be entirely neglected. By the resources of the Differential Calculus it is possible to calculate the motion of the particle A, as if it went through an infinite number of infinitely small diagonals of parallelograms. The great discoveries of Newton really arose from applying this method of calculation to the movements of the moon round the earth, which, while constantly tending to move onward in a straight line, is also deflected towards the earth by gravity, and moves through an elliptic curve, composed as it were of the infinitely small diagonals of infinitely small parallelograms. The mathematician, in his investigation of a curve, always in fact treats it as made up of a great number of short straight lines, and it



may even be doubtful whether he could treat it in any other manner. Nevertheless there is no error in the final results, because having obtained the formulæ flowing from this supposition, each straight line is then regarded as becoming infinitely small, and the polygonal line becomes undistinguishable from a perfect curve [z].

In abstract mathematical theorems the approximation to absolute truth is perfect, because we can treat of infinitesimals. In physical science, on the contrary, we treat of the least quantities which are perceptible. Nevertheless, while carefully distinguishing between these two different cases, we may fearlessly apply to both the principle of the superposition of small motions or effects. In physical science we have only to take care that the effects really are so small that any joint effect will be unquestionably imperceptible. Suppose, for instance, that there is some cause which alters the dimensions of a body in the ratio of 1 to $1 + \alpha$, and another cause which produces an alteration in the ratio of 1 to $1 + \beta$. If they both act at once the change will be in the ratio of 1 to $(1 + \alpha)(1 + \beta)$, or as 1 to $1 + \alpha + \beta + \alpha\beta$. But if α and β be both very small fractions of the total dimensions, $\alpha\beta$ will be yet far smaller and may be disregarded; the ratio of change is then approximately that of 1 to $1 + \alpha + \beta$, or the joint effect is the sum of the separate effects. Thus if a body were subjected to three strains at right angles to each other, the total change in the volume of the body would be approximately equal to the sum of the changes produced by the separate strains, provided that these are of very small amount. In like manner not only is the expansion of every solid and liquid substance by heat approximately proportional to the change of temperature, when this change is very small in amount, but the cubic

z Challis, 'Notes on the Principles of Pure and Applied Calculation,' 1860, p. 82.

expansion may also be considered as being three times as great as the linear expansion. For if the increase of temperature expands a bar of metal in the ratio of 1 to $1+\alpha$, and the expansion be equal in all directions, then a cube of the same metal would expand as 1 to $(1+\alpha)^3$, or as 1 to $1+3\alpha+3\alpha^2+\alpha^3$. When α is a very small quantity the third term $3\alpha^2$ will be imperceptible, and still more so the fourth term α^3. The coefficients of expansion of solids are in fact so small, and so imperfectly determined, that physicists seldom take into account their second and higher powers.

It is an universal and important result of these principles that all very small errors may be assumed to vary in simple proportion to their causes; a new reason why, in eliminating errors, we should first of all make them as small as possible. Let us suppose, with De Morgan, that there is a right-angled triangle of which the two sides containing the right angle are really of the lengths 3 and 4, so that the hypothenuse is $\sqrt{3^2+4^2}$ or 5. Now if in two measurements of the first side we commit slight errors, making it successively 4·001 and 4·002, then calculation will give the lengths of the hypothenuse as almost exactly 5·0008 and 5·00016, so that the error in the hypothenuse will seem to vary in simple proportion to that of the side, although it does not really do so with perfect exactness[a]. The logarithm of a number does not vary in proportion to that number—nevertheless we should find the difference between the logarithms of the numbers 100000 and 100001 to be almost exactly equal to that between the numbers 100001 and 100002. It is thus a general rule that very small differences between successive values of a function are approximately proportional to the small differences of the variable quantity.

[a] De Morgan's 'Differential Calculus.'

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Four Meanings of Equality.

Although it might seem that there are few terms more free from ambiguity than the term *equal*, yet scientific men do as a matter of fact employ it with four meanings, which it is very desirable to distinguish carefully. These meanings I may briefly describe as

(1) Absolute Equality.
(2) Sub-equality.
(3) Apparent Equality.
(4) Probable Equality.

By *absolute equality* we signify that which is complete and perfect to the last degree; but it is obvious that we can only know such equality in a theoretical or hypothetical manner. The areas of two triangles standing upon the same base and between the same parallels are absolutely equal. Hippocrates beautifully proved that the area of a lunula or figure contained between two segments of circles was absolutely equal to that of a certain right-angled triangle. As a general rule all geometrical and other elementary mathematical theorems involve absolute equality.

De Morgan proposed to describe as *sub-equal* those quantities which are equal within an infinitely small quantity, so that x is sub-equal to $x + dx$. The whole of the differential calculus may, as I apprehend it, be said to arise out of the neglect of infinitely small quantities; with this subject however we are not in this place much concerned. In mathematical science many other subtle distinctions may have to be drawn between kinds of equality, as De Morgan has shown in a remarkable memoir 'On Infinity; and on the Sign of Equality' [b].

Apparent equality is that with which physical science deals. Those magnitudes are practically equal which

b 'Cambridge Philosophical Transactions,' [1865] vol. xi, Part I.

differ only by an imperceptible quantity. To the carpenter anything less than the hundredth part of an inch is non-existent; there are few arts or artists to which the hundred-thousandth of an inch is of any account. Since all coincidence between physical magnitudes is judged by one or other sense, we must be restricted to a knowledge of apparent equality.

In reality even apparent equality is rarely to be expected. More commonly experiments will give only *probable equality*, that is results will come so near to each other that the difference may be ascribed to unimportant disturbing causes. Thus physicists often assume quantities to be equal provided that they fall within the limits of probable error of the processes employed. We cannot expect observations to agree with theory more closely than they agree with each other, as Newton remarked of his investigations concerning Halley's Comet.

Arithmetic of Approximate Quantities.

Considering that almost all the quantities which we treat in physical and social science are approximate only, it seems desirable that some attention should be paid in the teaching of arithmetic to the correct interpretation and treatment of approximate numerical statements. We ought carefully to distinguish between 2·5 when it means exactly two and a half, and when it means, as it usually does, anything between 2·45 and 2·55 It would be better in the latter case to write the number as 2·5 . . . . and we might then distinguish 2·50 . . . . as meaning anything between 2·495 . . . . and 2·505. When approximate numbers are added, subtracted, multiplied, or divided, it becomes a matter of some complexity to determine the degree of accuracy of the result. There are few persons, for instance, who could assert straightway that

the sum of the approximate numbers 34·70, 52·693, 80·1, is 167·5 *within less than* ·07. So far as I know Mr. Sandeman is the only mathematician who has traced out the rules of approximate arithmetic, and his directions are worthy of careful attention[c]. Although the accuracy of measurement has so much advanced since the time of Leslie, it is not superfluous to repeat his protest against the unfairness of affecting by a display of decimal fractions a greater degree of accuracy than the nature of the case requires and admits[d]. I have known a scientific man to register the barometer to a second of time when the nearest quarter of an hour would have been amply sufficient. Chemists often publish results of analysis to the ten-thousandth or even the millionth part of the whole, when in all probability the processes employed cannot be depended on beyond the hundredth part. It is seldom desirable to give more than one place of figures of uncertain amount; but it must be allowed that a nice perception of the degree of accuracy possible and desirable is requisite to save misapprehension and needless computation on the one hand, and to secure all attainable exactness on the other hand.

[c] Sandeman, 'Pelicotetics,' p. 214.

[d] Leslie, 'Inquiry into the Nature of Heat,' p. 505.

CHAPTER XXII.

QUANTITATIVE INDUCTION.

LET it be observed that we have not yet formally considered any processes of reasoning which have for their object to disclose general laws of nature expressed in quantitative formulæ or equations. We have been inquiring into the modes by which a phenomenon may be measured, and, if it be a composite phenomenon, may be resolved, by the aid of several measurements, into its component parts. We have also considered the precautions to be taken in the performance of observations and experiments in order that we may know what phenomena we really do measure and record. In treating of the approximate character of all observations, we have partially entered upon the subject of Quantitative Induction proper, but we must remember that no number of facts and observations can by themselves constitute science or general knowledge. Numerical facts, like other facts, are but the raw materials of knowledge, upon which our reasoning faculties must be exerted in order to draw forth the secret principles of nature. It is by an inverse process of reasoning that we can alone discover the mathematical laws to which varying quantities conform. By well-conducted experiments we gain a series of values of a variable, and a corresponding series of values of a variant, and we now want to know what mathematical function the variant is as regards the variable. In the usual progress of a science three questions will have to be answered as regards every important quantitative phenomenon :—

(1) Is there any constant relation between the variable and variant?

(2) What is the empirical formula expressing this relation?

(3) What is the rational formula expressing the law of nature involved?

Probable Connexion of Varying Quantities.

We find it stated in Mr. Mill's System of Logic[a] that, 'Whatever phenomenon varies in any manner whenever another phenomenon varies in some particular manner, is either a cause or an effect of that phenomenon, or is connected with it through some fact of causation.' This assertion may be considered true when it is interpreted with sufficient caution; but it might otherwise lead us into great errors. There is nothing whatever in the nature of things to prevent the existence of two variations which should apparently follow the same law, and yet have no connexion with each other. One binary star might be going through a revolution which, so far as we could tell, was of apparently equal period with that of another binary star, and according to the above rule the motion of one would be the cause of the motion of the other, which would not be really the case. Two astronomical clocks might conceivably be made so nearly perfect that, for several years, no difference could be detected, and we might then infer that the motion of one clock was the cause or effect of the motion of the other. This matter really requires the most careful discrimination. We must always bear in mind that the continuous quantities of space, time, force, &c., which we measure, are made up of an infinite number of infinitely small units. We may then meet with two variable phenomena which follow

[a] Book iii. chap. viii, § 6.

laws so nearly the same, that in no part of the variations open to our observation can any discrepancy be discovered. I grant that if two clocks could be shown to have kept *exactly* the same time during one year, or any finite interval of time, the probability would become infinitely high that there was a connexion between their motions. But it is apparent that we can never absolutely prove such coincidences to exist. Allow that we may observe a difference of one tenth or one hundredth of a second in their time, yet it is just possible that they were independently regulated so as to go together within less than that quantity of time. In short it would require either an infinitely long time of observation, or infinitely acute powers of measuring a discrepancy to decide positively whether two clocks were or were not in relation with each other.

A similar question actually occurs in the case of the moon's motion. We have absolutely no record that any other portion of the moon was ever visible to men than such as we now see. This fact sufficiently proves that within the historical period the rotation of the moon on its own axis has coincided with its revolutions round the earth. Does this coincidence prove a relation of cause and effect to exist between these motions? The answer must be in the negative, because there might have been so slight a discrepancy between the motions that there has not yet been time to produce any appreciable effect. There may nevertheless be a high probability of connexion.

The whole question of the relation of quantities thus resolves itself into one of probability. When we can only rudely measure a quantitative result, we can assign but slight importance to any correspondence. Because the brightness of two stars seems to vary in the same manner there is no appreciable probability that they have any relation with each other. Could it be shown that

their periods of variation were the same even to infinitely small quantities it would be certain, that is infinitely probable, that they were connected, however unlikely this might be on other grounds. The general mode of estimating such probabilities is identical with that applied to other inductive problems. Thus, if the two periods of variation were assigned by pure chance and entirely independently of each other, the probability would be about one in ten million that they would agree to the one ten-millionth part; but if the periods be observed to agree to less than that part then there is a probability of at least ten million to one in favour of the opposite hypothesis of connexion. That any two periods of variation should by chance become *absolutely equal* is infinitely improbable; hence if, in the case of the moon or any other change, we could prove absolute coincidence, we should have certainty of connexion[b]. With approximate measurements, which alone are within our power, we must hope for approximate certainty at the most.

The general principles of inference and probability, according to which we treat causes and effects varying in amount, are exactly the same as those by which we treated simple experiments. Continuous quantity, however, affords us an infinitely more extensive sphere of observation, because every different amount of cause, however little different, ought to be followed by a different amount of effect. If we can measure temperature to the one hundredth part of a degree centigrade, then even between 0° and 100° we have 10,000 possible distinct trials. If the precision of our measurements is increased, so that the one thousandth part of a degree can be appreciated, our trials may be increased tenfold. The probability of connexion will be proportional to the accuracy of our measurements.

[b] Laplace, 'System of the World,' transl. by Harte, vol. ii. p. 366.

When we have the power of varying the quantity of a cause entirely at our will it is easy to discover whether a certain effect is due to that cause or not. We can then make as many regular or irregular changes as we like, and it is quite incredible that the supposed effect should by chance go through exactly the corresponding series of changes unless by dependence. Thus, if we have a bell ringing *in vacuo*, the sound increases as we let in the air, and it decreases again as we exhaust the air. Tyndall's singing flames evidently obeyed the directions of his own voice; and Faraday when he discovered the relation of magnetism and light found that, by making or breaking or reversing the current of the electro-magnet, he had complete command over a ray of light, proving beyond all reasonable doubt the dependence of cause and effect. In such cases it is the perfect coincidence in time between the change in the effect and that in the cause which raises a high improbability of casual coincidence.

It is by a very simple case of variation that we infer the existence of a material connexion between two bodies moving with exactly equal velocity, such as the locomotive engine and the train which follows it. Elaborate observations were requisite before astronomers could all be convinced that the red hydrogen flames seen during solar eclipses belonged to the sun, and not to the moon's atmosphere as Flamsteed assumed. As early as 1706, Captain Stannyan noticed a blood red streak in an eclipse which he witnessed at Berne, and he asserted that it belonged to the sun; but his opinion was not finally established until photographs of the eclipse in 1860, taken by Mr. De la Rue, showed that the moon's dark body gradually covered the red prominences on one side, and uncovered those on the other, in short, that these prominences moved precisely as the sun moved and not as the moon moved.

Even when we have no means of accurately measuring the variable quantities we may yet be convinced of their connexion, if one always varies perceptibly at the same time as the other. Fatigue increases with exertion; hunger with abstinence from food; desire and degree of utility decrease with the quantity of commodity consumed. We know that the sun's heating power depends upon his height in the sky; that the temperature of the air falls in ascending a mountain; that the earth's crust is found to be perceptibly warmer as we sink mines into it; we infer the direction in which a sound comes from the change of loudness as we approach or recede. The facility with which we can time after time observe the increase or decrease of one quantity with another sufficiently shows the connexion, although we may be unable to assign any precise law of relation. The probability in such cases depends upon frequent coincidence in time.

Empirical Mathematical Laws.

It is important to acquire a clear comprehension of the part which is played in scientific investigation by empirical formulæ and laws. If we have a table containing certain values of a variable and the corresponding values of the variant, there are certain mathematical processes by which we can infallibly discover a mathematical formula yielding numbers in more or less exact agreement with the table. We may generally assume that the quantities will approximately conform to a law of the form

$$y = A + Bx + Cx^2,$$

in which x is the variable and y the variant. We can then select from the table three values of y, and the corresponding values of x; inserting them in the equation, we obtain three equations by the solution of which we gain the values of A, B, and C. It will be found as a

general rule that the formula thus obtained yields the other numbers of the table to a considerable degree of approximation.

In many cases even the second power of the variable will be unnecessary; thus Regnault found that the results of his elaborate inquiry into the latent heat of steam at different pressures were represented with sufficient accuracy by the empirical formula

$$\lambda = 606{\cdot}5 + 0{\cdot}305\ t,$$

in which λ is the total heat of the steam, and t the temperature[c]. In other cases it may be requisite to include the third power of the variable. Thus physicists assume the law of the dilatation of liquids to be of the form

$$\delta_t = a\,t + b\,t^2 + c\,t^3,$$

and they calculate from results of observation the values of the three constants a, b, c, which are usually small quantities not exceeding one hundredth part of a unit, but requiring to be determined with great accuracy[d]. Theoretically speaking, this process of empirical representation might be applied with any degree of accuracy; we might include still higher powers in the formula, and with sufficient labour obtain the values of the constants, by using an equal number of experimental results.

In a similar manner all periodic variations may be represented with any required degree of accuracy by formulæ involving the sines and cosines of angles and their multiples. The form of any tidal or other wave may thus be expressed, as Sir G. B. Airy has explained[e]. Almost all the phenomena registered by meteorologists are periodic in character, and when freed from disturbing causes may be embodied in empirical formulæ. Bessel has given a

[c] 'Chemical Reports and Memoirs,' Cavendish Society, p. 294.

[d] Jamin, 'Cours de Physique,' vol. ii. p. 38.

[e] 'On Tides and Waves,' Encyclopædia Metropolitana, p. 366*.

rule by which from any regular series of observations we may, on the principle of the method of least squares, calculate out with a moderate amount of labour a formula expressing the variation of the quantity observed, in the most probable manner. In meteorology three or four terms are usually sufficient for representing any periodic phenomenon, but the calculation might be carried to any higher degree of accuracy. As the details of the process have been described by Sir John Herschel in his admirable treatise on Meteorology[f], I need not further enter into them.

The reader might be tempted to think that in these processes of calculation we have an infallible method of discovering inductive laws, and that my previous statements (Chap. VII.) as to the purely tentative and inverse character of the inductive process are negatived. Were there indeed any general method of inferring laws from facts it would overturn my statement, but it must be carefully observed that these empirical formulæ do not coincide with natural laws. They are only approximations to the results of natural laws founded upon the general principles of approximation. It has already been pointed out that however complicated be the nature of a curve we may examine so small a portion of it, or we may examine it with such rude means of measurement, that its divergence from an elliptic curve will not be apparent. As a still ruder approximation a portion of a straight line will always serve our purpose; but if we need higher precision a curve of the third or fourth degree will almost certainly be sufficient. Now empirical formulæ really represent these approximate curves, but they give us no information as to the precise nature of the curve itself to which we are approximating. In another mode of expression we may say that we do not learn what function

[f] 'Encyclopædia Britannica,' art. *Meteorology*. Reprint §§ 152–156.

the variant is of the variable, but we obtain another function which, within the bounds of our observation, gives nearly the same series of values.

Discovery of Rational Formulæ.

Let us now proceed to consider the modes in which from numerical results we can establish the actual relation between the quantity of the cause and that of the effect. What we want is a *rational* formula or function, which may exhibit the *reason* or exact character and origin of the law in question. There is no word more frequently used by mathematicians than the word *function*, and yet it is difficult to define its meaning with perfect accuracy. Originally it meant performance or execution, being equivalent to the Greek λειτουργία or τέλεσμα. Mathematicians at first used it to mean *any power of a quantity*, but afterwards generalized it so as to include 'any quantity formed in any manner whatsoever from another quantity[8].' Any quantity, then, which depends upon and varies with another quantity may be called a function of it, and either may be considered a function of the other.

Given the quantities, we want the function of which they are the values. It may first of all be pointed out that simple inspection of the numbers cannot as a general rule disclose the function. In an earlier part of this work (vol. i. p. 142) I put before the reader certain numbers, and requested him to point out the law which they obey, and the same question will have to be asked in every case of quantitative induction. There are perhaps three methods, more or less distinct, by which we may hope to obtain an answer:

(1) By purely haphazard trial.

(2) By noting the general character of the variation of

[8] Lagrange, 'Leçons sur le Calcul des Fonctions,' 1806, p. 4.

the quantities, and trying by preference functions which give a similar form of variation.

(3) By deducing from previous knowledge the form of the function which is most likely to suit.

Having certain numerical results we are always at perfect liberty to invent any kind of mathematical formula we like, and then try whether, by the suitable selection of values for the unknown constant quantities we can make it give the required results. If ever we fall upon a formula which does so, to a fair degree of approximation, there is a presumption in favour of its being the true function, although there is no certainty whatever in the matter. In this way I happened to discover a simple mathematical law which closely agreed with the results of certain experiments on muscular exertion. This law was afterwards shown by Professor Haughton to be the true rational law according to his theory of muscular action [h].

But the chance of succeeding in this manner is usually very small. The number of possible functions is certainly infinite, and even the number of comparatively simple functions is so very large that the probability of falling upon the correct one by mere chance is very slight. Let the reader observe that even when we can thus obtain the law it is by a deductive process, not by showing that the numbers give the law, but that the law gives the numbers.

In the second place, we may, by a survey of the numbers, gain a general notion of the kind of law they are likely to obey, and we may be much assisted in this process by drawing them out in the form of a curve, as will be presently considered. We can in this way ascertain with some probability whether the curve is likely to

[h] Haughton, 'Principles of Animal Mechanics,' 1873, pp. 444–450. *Nature*, 30th of June, 1870, vol. ii. p. 158.

be a closed one, or whether it has infinite branches; whether such branches are asymptotic, that is, approach indefinitely towards straight lines; whether it is logarithmic in character, or trigonometric. This indeed we can only do if we remember the results of previous investigations. The process is still inversely deductive, and consists in noting what laws gave particular curves, and then inferring inversely that such curves belong to such laws. If we can in this way discover the class of functions to which the required law belongs, our chances of complete success are much increased, because our haphazard trials are now reduced within a narrower sphere. But, unless we have almost the whole curve before us, the identification of its character must be a matter of great uncertainty; and if, as in most physical investigations, we have a mere fragment of the curve, the assistance given would be quite illusory. Curves of almost any character can be made to approximate to each other for a limited extent, so that it is only by a kind of *divination* that we can fall upon the actual function, unless we have theoretical knowledge of the kind of function applicable to the case.

When we have once obtained what we believe to be the correct form of function, the remainder of the work is mere mathematical computation to be performed infallibly according to fixed rules[i], which include those employed in the determination of empirical formulæ (vol. ii. p. 110). The function will involve two or three or more unknown constants, the values of which we need to determine by our experimental results. Selecting some of our results widely apart and nearly equidistant, we must form by means of them as many equations as there are constant quantities to be determined. The solution of these equations will then give us the constants required, and having

[i] See Jamin, 'Cours de Physique,' vol. ii. p. 50.

now the actual function we can try whether it gives with sufficient accuracy the remainder of our experimental results. If not, we must either make a new selection of results to give a new set of equations, and thus obtain a new set of values for the constants, or we must acknowledge that our form of function has been wrongly chosen. If it appears that the form of function has been correctly ascertained, we may regard the constants as only approximately accurate and may proceed by the Method of Least Squares (vol. i. p. 458) to determine the most probable values as given by the whole of the experimental results.

In most cases we shall find ourselves obliged to fall back upon the third mode, that is, anticipation of the form of the law to be expected on the ground of previous knowledge. Theory and analogical reasoning must be our guides. The general nature of the phenomenon will often indicate the kind of law to be looked for. If one form of energy or one kind of substance is being converted into another, we may expect the law of direct simple proportion. In one distinct class of cases the effect already produced influences the amount of the ensuing effect, as for instance in the cooling of a heated body, when the law will be of an exponential form. When the direction in which a force acts influences its action, trigonometrical functions must of course enter. Any force or influence which spreads freely through tridimensional space will be subject to the law of the inverse square of the distance. From such considerations we may sometimes arrive deductively and analogically at the general nature of the mathematical law required.

The Graphical Method.

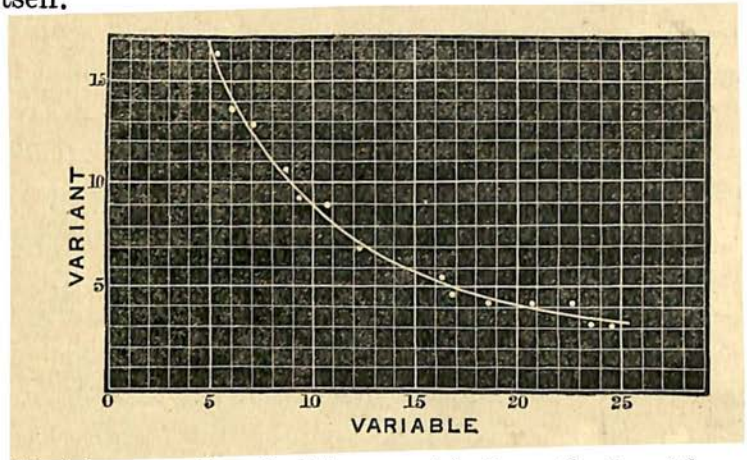
In endeavouring to discover the mathematical law obeyed by experimental results it is often necessary,

and almost always desirable, to call in the aid of space-representations. Every equation involving two variable quantities corresponds to some kind of plane curve, and every plane curve may be represented symbolically in an equation of a more or less complex character, containing two unknown quantities. Now in an experimental research we obtain a number of values of the variant corresponding to an equal number of values of the variable; but all the numbers are affected by more or less error, and the values of the variable will often be irregularly disposed. Even if the numbers were absolutely correct and disposed at regular intervals, there is, as we have seen, no direct mode of discovering the law, but the difficulty of discovery is much increased by the uncertainty and irregularity of the results.

Under such circumstances, the best mode of proceeding is to procure or prepare a paper divided into small equal rectangular spaces, a convenient size for the spaces being one-tenth of an inch square. The values of the variables being marked off along the scale formed by the lowest horizontal line, a point is marked for each corresponding value of the variant perpendicularly above that of the variable, and at such a height as corresponds to the amount of the variant.

The exact scale of the drawing is not of much importance, but it may require to be adjusted according to circumstances, and different values must often be attributed to the upright and horizontal divisions, so as to make the variations conspicuous, but not excessive. If now a curved line be drawn through all the extremities of the ordinates, it will probably exhibit many irregular inflections, owing to the errors which affect all the numbers. But, when the results are numerous, it soon becomes apparent which results are more divergent than others, and guided by a so-called *sense of continuity*, it becomes pos-

sible to trace a line among the points which will approximate to the true law more nearly than the points themselves. The accompanying figure sufficiently explains itself.



Perkins employed this graphical method with much care in exhibiting the results of his experiments on the compression of water[k]. The numerical results were marked upon a sheet of paper very exactly ruled at intervals of one-tenth of an inch, and the original marks were left in order that the reader might judge of the correctness of the curve drawn, or choose another for himself. Regnault carried the method to perfection by laying off the points with a small screw dividing engine[l]; and he then formed a table of results by drawing a continuous curve, and measuring its height for equidistant values of the variable.

Not only does a curve drawn in this manner enable us to assign by measurement numerical results more free from accidental errors than any of the numbers obtained directly from experiment, but the form of the curve sometimes indicates the class of functions to which our results belong

k 'Philosophical Transactions,' 1826, p. 544.

l Jamin, 'Cours de Physique,' vol. ii. p. 24, &c.

Engraved sheets of paper ready prepared for the drawing of curves may be obtained from Mr. Stanford, at 6 and 7 Charing Cross, or from Messrs. W. and A. K. Johnston, of London and Edinburgh. When we do not require great accuracy, paper ruled by the common machine-ruler into equal squares of about one-fifth or one-sixth of an inch square will serve well enough. I have found Vere Foster's Exercise Book, No. 12 [m], which is ruled in this way, very useful for statistical or other numerical purposes. I have also met with engineers' and surveyors' memorandum books ruled with one-twelfth inch squares. When a number of complicated curves have to be drawn, I have found it best to rule a good sheet of drawing paper with lines carefully adjusted at the most convenient distances, and then to prick the points of the curve through it upon another sheet fixed underneath. In this way we obtain an accurate curve upon a blank sheet, and need only introduce such division lines as are requisite to the understanding of the curve.

In some cases our numerical results will correspond, not to the height of single ordinates, but to the area of the curve between two ordinates, or the average height of ordinates between certain limits. If we measure, for instance, the quantities of heat absorbed by water when raised in temperature from 0° to 5°, from 5° to 10°, and so on, these quantities will really be represented by *areas* of the curve denoting the specific heat of water; and, since the specific heat varies continuously between every two points of temperature, we shall not get the correct curve by simply laying off the quantities of heat at the mean temperatures, namely $2\frac{1}{2}°$, $7\frac{1}{2}°$, and so on. Mr. J. W. Strutt has shown that if we have drawn such an incorrect curve, we can with little trouble correct it by a simple

[m] Published by Whittaker & Co., London.

geometrical process, and obtain to a very close approximation the true ordinates instead of those denoting areas[n].

Interpolation and Extrapolation.

When we have by experiment obtained two or more numerical results, and endeavour, without further resort to experiment, to infer and calculate intermediate results, we are said to *interpolate*. If we wish to assign by reasoning results lying beyond the limits of experiment, we may be said, using an expression of Sir George Airy, to *extrapolate*. These two operations are to a certain extent the same in principle, but differ in practicability. It is a matter of great scientific importance to apprehend precisely how far we can interpolate or extend experimental results by extrapolation, and on what grounds we proceed.

In the first place, if the interpolation is to be more than empirical and speculative, we must have not only the experimental results, but the laws which they obey—we must in fact go through the complete process of scientific investigation. Having discovered the laws of nature applying to the case, and verified them by showing that they agree with the experiments in question, we are then in a fair position to anticipate the results of any similar experiments. Our knowledge even now is not certain, because we cannot completely prove the truth of any assumed law, and we cannot possibly exhaust all the circumstances which may more or less affect the result. Even at the best then our interpolations will partake of the want of certainty and precision attaching to all our knowledge of nature. Yet having the supposed laws, our

[n] J. W. Strutt, 'On a correction sometimes required in curves professing to represent the connexion between two physical magnitudes.' 'Philosophical Magazine,' 4th Series, vol. xlii. p. 441.

results will be as sure and accurate as any we can attain to. But such a complete procedure is more than we generally mean by interpolation, which generally denotes the employment of some general method of estimating in a merely approximate and probable manner the results which might have been expected independently of any complete theoretical investigation.

Regarded in this light, interpolation is in reality an indeterminate problem. From given values of a function it is impossible to determine that function; for we can always invent an infinite number of functions which would give those values if we are not restricted by any other conditions, just as through a given series of points we can always draw an infinite number of curves, if we may diverge between or beyond the points into bends and cusps as we think fit[o]. In any process of interpolation we must in fact be guided more or less by *à priori* considerations; we must know, for instance, whether or not periodical fluctuations are to be expected, and we must be guided accordingly in the choice of mathematical formulæ. Supposing, for the present, that the phenomenon is non-periodic, we next proceed to assume that the function can be expressed in a limited series of the powers of the variable. The number of powers which can be included depends upon the number of experimental results available, and must be at least one less than this number. By processes of calculation, which have been already alluded to in the section on empirical formulæ, we can then calculate the coefficients of the powers, and obtain an empirical formula which will give the required intermediate results. In reality, then, we return to the methods treated under the head of approximation and empirical formulæ; and interpolation, as commonly understood, consists in assum-

[o] Herschel, 'Appendix to Translation of Lacroix' Differential Calculus,' p. 551.

ing that a curve of simple character is to pass through certain determined points. If we have, for instance, two experimental results, and only two, we must assume that the curve is a straight line; for the parabolas which can be passed through two points are infinitely various in magnitude, and quite indeterminate. One straight line alone can pass through two points, and it will have an equation of the form $y = mx + n$, the constant quantities of which can be readily determined from two results. Thus, if the two values for x, 7 and 11, give the values for y, 35 and 53, the solution of two simple equations gives $y = 4{\cdot}5 \times x + 3{\cdot}5$ as the equation, and for any other value of x, for instance 10, we get a value of y, 48·5. When we take an exactly intermediate value of x, namely 9, this process yields a simple mean result, namely 44. Three experimental results being given, we may assume that they fall upon a portion of a parabola, and simple algebraic calculation readily gives the position of any intermediate point upon the parabola. Concerning the process of interpolation as practised in the science of meteorology the reader will find some directions in the French edition of Kæmtz' Meteorology p.

When we have, either directly by experiment or by the use of a curve, a series of values of the variant for exactly equidistant values of the variable, it is often very instructive to take the differences between each value of the variant and the next, and then the differences between those differences, and so on. If any series of differences approaches closely to zero it is an indication that the numbers may be correctly represented by a finite empirical formula; if the nth differences are zero, then the formula will contain only the first $n-1$ powers of the variable. Indeed we may sometimes obtain by the Cal-

p 'Cours complet de Météorologie,' traduit par Martins, Note A, du Traducteur, p. 449.

culus of differences a correct empirical formula; for if p be the first term of the series of values, and Δp, $\Delta^2 p$, $\Delta^3 p$, &c., be the first number in each column of differences, then the mth term of the series of values will be

$$p + m\Delta p + m\frac{m-1}{2}\Delta^2 p + m\frac{m-1}{2}\frac{m-2}{3}\Delta^3 p + \text{ &c.}$$

A closely equivalent but more practicable formula for interpolation by differences, as devised by Lagrange, will be found in Thomson and Tait's 'Elements of Natural Philosophy,' p. 115.

If no column of differences shows any tendency to become zero throughout, it is an indication that the law is of a more complicated, for instance of an exponential, character, so that it cannot be correctly represented in a formula involving only a few powers of the variable. Dr. J. Hopkinson has lately suggested another method of arithmetical interpolation[q], which is intended to avoid much that is arbitrary in the graphical method. His process will yield the same results in all hands, but he remarks that it has no theoretical basis to rest on.

So far as we can infer the results likely to be obtained by variations beyond the limits of experiment, we must proceed upon the same principles. If possible we must detect the exact laws in action, and then trust to them as a guide when we have no experience. If not, an empirical formula of exactly the same character as those employed in interpolation is our only resource. But the reader must carefully observe that to extend our inference far beyond the limits of experience is exceedingly unsafe. Our knowledge is at the best only approximate, and takes no account of very small tendencies. Now it may, and in fact usually will, happen, that tendencies small within our limits of

q 'On the Calculation of Empirical Formulæ.' 'The Messenger of Mathematics,' New Series, No. 17, 1872.

observation will become perceptible or great under extreme circumstances. When the variable in our empirical formula is small, we are justified in overlooking the existence of higher powers, and taking only two or three of them. But as the variable increases, those higher powers gain in importance, and in time will yield the principal part of the value of the function.

This is no mere theoretical inference. Excepting the few great primary laws of nature, such as the law of gravity, the conservation of energy, &c., there is hardly any natural law which we can trust in circumstances widely different from those with which we are practically acquainted. From the expansion or contraction, fusion or vaporisation of substances by heat at the surface of the earth, we can form a most imperfect notion of what would happen near the centre of the earth, where the pressure must almost infinitely exceed anything possible in our experiments. The physics of the earth again give us a feeble, and probably often a misleading, notion of a body like the sun, in most parts of which an almost inconceivably high temperature is united with an inconceivably high pressure. If, as is probable, there are in the realms of space many nebulæ consisting of incandescent and unoxydized vapours of metals and other elements, so highly heated perhaps that chemical composition is out of the question, we are hardly able to treat them as subjects of scientific inference. Hence arises the great importance of any experiments in which we can investigate the properties of substances under extreme circumstances of cold or heat, density or rarity, intense electric excitation, &c. It should be observed that this insecurity in extending our inferences wholly arises from the purely approximate character of our measurements. Had we the power of appreciating indefinitely small quantities, we should by the principle of continuity discover some

trace of every change which a substance could undergo under unattainable circumstances. By observing, for instance, the tension of aqueous vapour between 0° and 100° C., we ought theoretically to be able to infer its tension at every other temperature; but this is out of the question because we cannot really ascertain the law precisely between those temperatures.

Many instances might be given to show that laws which appear to represent correctly the results of experiments within certain limits altogether fail beyond those limits. The experiments of Roscoe and Dittmar, on the absorption of gases in water[r] afford many interesting illustrations, especially in the case of hydrochloric acid, the quantity of which dissolved in water under different pressures follows very closely a linear law of variation, from which however it diverges very widely at low pressures[s]. Sir J. Herschel having deduced from various recorded observations of the double star γ Virginis, an elliptic orbit for the motion of one component round the centre of gravity of both, found that for a certain time the motion of the star agreed very well with this orbit. Nevertheless a divergence began to appear by degrees, and after a time became so great that an entirely new orbit, of more than double the linear dimensions of the old one, had ultimately to be adopted[t].

Illustrations of Empirical Quantitative Laws.

Although our chief object in every quantitative inquiry must be to discover the exact or rational formulæ, expressing the general laws of nature applying to the subject, it is instructive to observe in how many important branches

[r] Watts's 'Dictionary of Chemistry,' vol. ii. p. 790.

[s] 'Quarterly Journal of the Chemical Society,' vol. viii. p. 15.

[t] 'Results of Observations at the Cape of Good Hope,' p. 293.

of science, no precise laws have yet been detected. The tension of aqueous vapour at different temperatures has been determined by a succession of eminent experimentalists, Dalton, Kæmtz, Dulong, Arago, Magnus, and Regnault, and by the last mentioned the measurements were conducted with all accuracy apparently attainable at present. Yet no incontestible general law has been established. Several functions have been proposed to express the elastic force of the vapour as depending on the temperature. The first general form is that of Young, namely $F = (a + b\ t)^m$, in which a, b and m are unknown quantities to be determined by comparison with observation. Roche has proposed, on theoretical grounds, a complicated formula of an exponential form, and a third form of function is that of Biot, as follows—$\log F = a + ba^t + c\ \beta^{t^u}$. I mention these formulæ particularly, because they well illustrate the feeble powers of empirical inquiry. None of the formulæ can be made to correspond exactly with experimental results, and the last two forms correspond nearly equally well. But there is very little probability that the real law has been reached, and it is highly unlikely that it will be discovered except by deduction from mechanical theory.

The same remarks may be made upon any other laws except those of the most simple character. A vast amount of the most ingenious labour has been spent upon the discovery of some general law of atmospheric refraction. Tycho Brahe and Kepler commenced the inquiry: Cassini first formed a table of refractions, calculated on theoretical grounds: Newton entered into some profound investigations upon the subject: Brooke Taylor, Bouguer, Simpson, Bradley, Mayer, and Kramp successively attacked the question, which is of the highest practical importance as regards the correction of astronomical observations. Laplace next

[u] Jamin, 'Cours de Physique,' vol. ii. p. 138.

laboured on the subject without exhausting it, and Brinkley and Ivory have since treated it. A closely connected problem, that regarding the relation between the pressure and elevation in different strata of the atmosphere, has received the attention of a long succession of physicists and was most carefully investigated by Laplace. Yet no invariable and general law has been detected. The same may be said concerning the law of human mortality; abundant statistics on this subject are available, and many hypotheses more or less satisfactory have been put forward as to the general form of the curve of mortality, but it seems to be impossible to discover more than an approximate law.

It may perhaps be urged that in such subjects no single invariable law can be expected. The atmosphere may be divided into several variable strata which by their unconnected changes frustrate the exact calculations of astronomers. Human life may be subject at different ages to a succession of different influences incapable of reduction under any one law. The results observed may in fact be aggregates of an immense number of separate results each governed by their own separate laws, so that the subjects may be complicated beyond the possibility of complete resolution by empirical methods. This is certainly true of the mathematical functions which must some time or other be introduced into the science of political economy.

Simple Proportional Variation.

When we first treat numerical results in any novel kind of investigation, our impression will probably be that one quantity varies in *simple proportion* to another, so as to obey the law $y = mx + n$. We must learn to distinguish carefully between the cases where this proportionality is really, and where it is only apparently true. When con-

sidering the principles of approximation we found that a small portion of any curve will appear to be a straight line. Whenever our modes of measurement are comparatively rude, we must expect to be unable to detect the curvature. Thus Kepler made meritorious attempts to discover the law of refraction, and he slightly approximated to it when he observed that the angles of incidence and refraction *if small* bear a constant ratio to each other. Angles when small are very nearly as their sines, so that he reached an approximate result of the true law. Cardan assumed, probably as a mere guess, that the force required to sustain a body on an inclined plane was simply proportional to the angle of elevation of the plane. This is approximately the case when the angle is very small, and it becomes true again when the angle is a right angle; but in reality the law is much more complicated, the power required being proportional to the sine of the angle. The early thermometer-makers were quite unaware whether the expansion of mercury was exactly proportional or not to the heat communicated to it, and it is only in the present century that we have learnt it to be not so. We now know that even gases obey the law of uniform expansion by heat only in an approximate manner. Until some reason to the contrary is shown, we should do well to look upon every law of simple proportion as only provisionally true.

Nevertheless, there are many of the most important laws of nature which are in the form of simple proportions. Wherever a uniform cause acts in independence of its previous effects, we may expect this relation. Thus, an accelerating force acts equally upon a moving and a motionless body. Hence the velocity produced is always in simple proportion to the force, and also to the duration of its uniform action. As gravitating bodies never interfere with each other's gravity, this force is in direct

simple proportion to the mass of each of the attracting bodies, the mass being measured by, or proportional to inertia. Similarly, in all cases of 'direct unimpeded action,' as Sir J. Herschel has remarked [x], we may expect simple proportion to manifest itself. In such cases the equation expressing the relation may have the still simpler form $y = mx$.

A similar simple relation holds true wherever there is a conversion of one substance or form of energy into another. The quantity of chloride of silver is proportional to the quantity either of chlorine or silver. The amount of heat produced in friction is exactly proportional to the mechanical energy absorbed. It was experimentally proved by Faraday that 'the chemical power of the current of electricity is in direct proportion to the quantity of electricity which passes.' When an electric current is produced, the quantity of electric energy is simply proportional to the weight of metal dissolved. If electricity is turned into heat, there is again simple proportion. Wherever, in fact, one thing is but another thing with a new aspect, we may expect to find the law of simple proportion. It is only among the most elementary causes and effects that this simple relation will hold true. Simple conditions do not, generally speaking, produce simple results. The planets move in approximate circles round the sun, but the apparent motions, as seen from the earth, are so various, that men have not believed in such a simple view of the matter for more than about two centuries and a half. All those motions, again, are summed up in the law of gravity, of no great complexity, yet men never have, and never can be, able to exhaust the complications of action and reaction, even among a small number of planets. We should be on our guard against a tendency to assume that

x 'Preliminary Discourse,' &c. p. 152.

the connexion of cause and effect is one of direct proportion. Bacon reminds us of the woman in Æsop's fable, who expected that her hen, with a double measure of barley, would lay two eggs a day instead of one, whereas it thereby grew fat, and ceased to lay any eggs at all.

CHAPTER XXIII.

THE USE OF HYPOTHESIS.

If the views of induction upheld in this work be correct, all inductive investigation consists in a marriage of hypothesis and experiment. When facts are already in our possession, we frame an hypothesis to explain their mutual relations, and by the success or non-success of this explanation is the value of the hypothesis to be entirely judged. In the framing and deductive treatment of such hypotheses, we must avail ourselves of the whole body of scientific truth already accumulated, and when once we have obtained a probable hypothesis, we must not rest until we have verified it by comparison with new facts. By deductive reasoning and calculation, we must endeavour to anticipate such new phenomena, especially those of a singular and exceptional nature, as would necessarily happen if the hypothesis be true. Out of the infinite number of observations and experiments which are possible at every moment, theory must lead us to select those few critical ones which are suitable for confirming or negativing our anticipations.

This work of inductive investigation cannot be guided by any system of precise and infallible rules, like those of deductive reasoning. There is, in fact, nothing to which we can apply rules of method, because the laws of nature to be treated must be in our possession before we can treat them. If, indeed, there were any single rule of

inductive method, it would direct us to make an exhaustive arrangement of facts in all possible orders. Given a certain number of specimens in a museum, we might arrive at the best possible classification by going systematically through all possible classifications, and, were we endowed with infinite time and patience, this would be an effective method. It doubtless is the method by which the first few simple steps are taken in every incipient branch of science. Before the dignified name of science is applicable, some coincidences will chance to force themselves upon the attention. Before there was a science of meteorology, or any comprehension of the true conditions of the atmosphere, all observant persons learned to associate a peculiar clearness of the atmosphere with coming rain, and a colourless sunset with fine weather. Knowledge of this kind is called *empirical*, as seeming to come directly from experience; and there is doubtless a considerable portion of our knowledge which must always bear this character.

We may be obliged to trust to the casual detection of coincidences in those branches of knowledge where we are deprived of the aid of any guiding notions; but a very little reflection will show the utter insufficiency of haphazard experiment, when applied to investigations of a complicated nature. At the best, it will be the simple identity, or partial identity, of classes, as illustrated in pp. 146–154 of the first volume, which can be thus detected. It was pointed out that, even when a law of nature involves only two circumstances, and there are one hundred distinct circumstances which may possibly be connected, there will be no less than 4950 pairs of circumstances between which a coincidence may exist. When a law involves three or more circumstances, the possible number of coincidences becomes vastly greater still. When considering, again, the subject

of combinations and permutations, it became apparent that we could never cope with the possible variety of nature. An exhaustive examination of the metallic alloys, or chemical compounds which can be formed, was found to be out of the question (vol. i. p. 218). It is on such considerations that we can explain the very small additions made to our knowledge by the alchemists. Many of them were men of the greatest acuteness, and their indefatigable labours were pursued through many centuries. A few of the more common compounds and phenomena were discovered by them, but a true insight into the principles of nature, now enables chemists to discover far more useful facts in a single year than were yielded by the alchemists during many centuries. There can be no doubt that Newton was really an alchemist, and often spent his days and nights in laborious experiments. But in trying to discover the secret by which gross metals might be rendered noble, his lofty powers of deductive investigation were wholly useless. Deprived of all guiding clues, his experiments must have been, like those of all the alchemists, purely tentative and haphazard. While his hypothetical and deductive investigations have given us the true system of nature, and opened the way in almost every one of the great branches of natural philosophy, the whole results of his tentative experiments are comprehended in a few happy guesses, given in his celebrated 'Queries.'

Even when we are engaged in apparently passive observation of a phenomenon, which we cannot modify experimentally, it is advantageous that our attention should be guided by some theoretical anticipations. A phenomenon which seems simple is, in all probability, really complex, and unless the mind is actively engaged in looking for particular details, it is quite likely that the most critical circumstances will be passed over. Bessel

regretted that no distinct theory of the constitution of comets had guided his observations of Halley's comet[a]; in attempting to verify or refute any good hypothesis, not only would there have been a chance of establishing a true theory, but if confuted, the very confutation would probably have involved a large store of useful observations.

It would be an interesting work, but one which I cannot undertake, to trace out the gradual reaction which has taken place in recent times against the purely empirical, or Baconian, theory of induction. Francis Bacon, seeing the futility of the scholastic logic, which had long been predominant, asserted that the accumulation of facts and the careful and orderly abstraction of axioms, or general laws from them, constituted the true method of induction. This method, as far as we can gather its exact nature from Bacon's writings, would correspond to the process of exhaustive examination and classification to which I have just alluded. The value of this method might be estimated historically by the fact that it has not been followed by any of the great masters of science. Whether we look to Galileo, who preceded Bacon, to Gilbert, his contemporary, or to Newton and Descartes, his successors, we find that discovery was achieved by the exactly opposite method to that advocated by Bacon. Throughout Newton's works, as I shall more fully show in succeeding pages, we find deductive reasoning wholly predominant, and experiments are employed, as they should be, to confirm or refute hypothetical anticipations of nature. In my 'Elementary Lessons in Logic' (p. 258), I stated my belief that there was no kind of reference to Bacon in Newton's works. I have since found that Newton does once or twice employ the

a Tyndall, 'On Cometary Theory,' Philosophical Magazine, April, 1869. 4th Series, vol. xxxvii. p. 243.

expression *experimentum crucis* in his 'Opticks,' but this is the only expression, so far as I am aware, which could indicate on the part of Newton direct or indirect acquaintance with Bacon's writings [b].

Other great physicists of the same age were equally prone to the use of hypotheses rather than the blind accumulation of facts in the Baconian manner. Hooke emphatically asserts in his posthumous work on Philosophical Method, that the first requisite of the Natural Philosopher is readiness at guessing the solution of many phenomena and making queries. 'He ought to be very well skilled in those several kinds of philosophy already known, to understand their several hypotheses, suppositions, collections, observations, &c., their various ways of ratiocinations and proceedings, the several failings and defects, both in their way of raising, and in their way of managing their several theories: for by this means the mind will be somewhat more ready at guessing at the solution of many phenomena almost at first sight, and thereby be much more prompt at making queries, and at tracing the subtlety of Nature, and in discovering and searching into the true reason of things.'

We find Horrocks, again, than whom no one was more filled with the scientific spirit, telling us how he tried theory after theory in order to discover one which was in accordance with the motions of Mars [c]. It might readily be shown again that Huyghens, who possessed one of the most perfect philosophical intellects, followed the deductive process combined with continual appeal to experiment, with a skill closely analogous to that of Newton. As to Descartes and Leibnitz, their investigations were too much opposed to the Baconian rules, since they too often

[b] See 'Philosophical Transactions,' abridged by Lowthorp. 4th edit. vol. i. p. 130.

[c] Horrocks, 'Opera Posthuma' (1673), p. 276.

adopted hypothetical reasoning to the exclusion of experimental verification. Throughout the eighteenth century science was supposed to be advancing by the pursuance of the Baconian method, but in reality hypothetical investigation was the main instrument of progress. It is only in the present century that physicists began to recognise this truth. So much opprobrium had been attached by Bacon to the use of hypotheses, that we find Young speaking of them in an apologetic tone. 'The practice of advancing general principles and applying them to particular instances is so far from being fatal to truth in all sciences, that when those principles are advanced on sufficient grounds, it constitutes the essence of true philosophy[d]'; and he quotes cases in which Sir Humphry Davy trusted to his theories rather than his experiments. The late Sir John Herschel, who was both a practical physicist and an abstract logician, always entertained the deepest respect for Bacon, and made the 'Novum Organum' as far as possible the basis of his admirable 'Discourse on the Study of Natural Philosophy.' Yet we find him in Chapter VII fully recognising the part which the formation and verification of theories forms in the higher and more general investigations of physical science. The late Mr. J. S. Mill carried on the reaction by recognising as a distinct method the Deductive Method in which Ratiocination, that is, deductive reasoning, is employed for the discovery of new opportunities of testing and verifying a hypothesis. His main error consisted in the fact that throughout the other parts of his system he inveighed against the value of the deductive process, and even asserted from time to time that every process of reasoning is inductive. In fact Mill fell into much confusion in the use of the words induction and deduction, because he

[d] Young's Works, vol. i. p. 593.

failed to observe that the inverse use of deduction constitutes induction.

Even Francis Bacon was not wholly unaware of the value of hypothetical anticipation. In one or two places he incidentally acknowledges it, as when he remarks that the subtlety of nature surpasses that of reason, adding that 'axioms abstracted from particular facts in a careful and orderly manner, readily suggest and mark out new particulars.'

The true course of inductive procedure is that which has yielded all the more lofty and successful results of science. It consists in *Anticipating Nature*, in the sense of forming hypotheses as to the laws which are probably in operation; and then observing whether the combinations of phenomena are such as would follow from the laws supposed. The investigator begins with facts and ends with them. He uses such facts as are in the first place known to him in suggesting probable hypotheses; deducing other facts which would happen if a particular hypothesis is true, he proceeds to test the truth of his notion by fresh observations or experiments. If any result prove different from what he expects, it leads him either to abandon or to modify his hypothesis; but every new fact may give some new suggestion as to the laws in action. Even if the result in any case agrees with his anticipations, he does not regard it as finally confirmatory of his theory, but proceeds to test the truth of the theory by new deductions and new trials.

The investigator in such a process is assisted by the whole body of science previously accumulated. He may employ analogy, as I shall point out, to guide him in the choice of hypotheses. The manifold connexions between one science and another may give him strong clues to the kind of laws to be expected, and he thus always selects out of the infinite number of possible hypotheses those

which are, as far as can be foreseen at the moment, most probable. Each experiment, therefore, which he performs is that most likely to throw light upon his subject, and even if it frustrate his first views, it probably tends to put him in possession of the correct clue.

Requisites of a Good Hypothesis.

There will be no difficulty in pointing out to what conditions, or rather to what condition an hypothesis must conform in order to be accepted as valid and probable. That condition, as I conceive, is the single one of enabling us to infer the existence of phenomena which occur in our experience. *Agreement with fact is the one sole and sufficient test of a true hypothesis.*

Hobbes, indeed, has named two conditions which he considers requisite in an hypothesis, namely, (1) That it should be conceivable and not absurd; (2) That it should allow of phenomena being necessarily inferred. Boyle, in noticing Hobbes' views, proposed to add a third condition, to the effect that the hypothesis should not be inconsistent with any other truth or phenomenon of nature[e]. Of these three conditions, I am inclined to think that the first cannot be accepted, unless by *inconceivable* and *absurd* we mean self-contradictory or inconsistent with the laws of thought and nature. I shall have to point out that some of the most sure and satisfactory theories involve suppositions which are wholly *inconceivable* in a certain sense of the word, because the mind cannot sufficiently extend its ideas to frame a notion of the actions supposed to exist. That the force of gravity should act instantaneously between the most distant parts of the planetary system, or that a ray of violet light should consist of

[e] Boyle's 'Physical Examen,' p. 84.

about 700 billions of vibrations in each second, are statements of an inconceivable and absurd character in one sense; but they are so far from being opposed to fact that we cannot on any other suppositions account for the phenomena observed. But if an hypothesis involve self-contradiction, or is inconsistent with known laws of nature, it is so far self-condemned. We cannot even apply processes of deductive reasoning to a self-contradictory notion; and being entirely opposed to the most general and certain laws known to us, the primary laws of thought, it thereby conspicuously fails to agree with facts. Since nature, again, is never self-contradictory, we cannot at the same time accept two theories which lead to contradictory results. If the one agrees with nature, the other cannot. Hence if there be a law which we believe with high probability to be verified in observation, we must not frame an hypothesis in conflict with it, otherwise the hypothesis will necessarily be in disagreement with observation. Since no law or hypothesis is proved, indeed, with absolute certainty, there is always a chance, however slight, that the new hypothesis may displace the old one; but the greater the probability which we assign to that old hypothesis, the greater must be the evidence required in favour of the new and conflicting one. A decisive *experimentum crucis* to negative the one, and establish the other, will probably be requisite to allay the strife.

I am inclined to assert, then, that there is but one test of a good hypothesis, namely, *its conformity with observed facts;* but this condition may be said to involve, at the same time, three minor conditions, nearly equivalent to those suggested by Hobbes and Boyle, namely:—

(1) That it allow of the application of deductive reasoning and the inference of consequences.

(2) That it do not conflict with any laws of nature, or of mind, which we hold as true.

(3) That the consequences inferred do agree with facts of observation.

The First Requisite—Possibility of Deductive Reasoning.

As the truth of an hypothesis is to be proved by its conformity with fact, the first condition is that we be able to apply methods of deductive reasoning, and learn what should happen according to such an hypothesis. Even if we could imagine an object acting according to laws wholly unknown in other parts of nature, it would be useless to do so, because we could never decide whether it existed or not. We can only infer what would happen under supposed conditions by applying what knowledge we possess of nature to those conditions. Hence, as Boscovich truly said, we are to understand by hypotheses 'not fictions altogether arbitrary, but suppositions conformable to experience or analogy.' It follows that every hypothesis worthy of consideration must suggest some likeness, analogy, or common law, acting in two or more things. If, in order to explain certain facts, a, a', a'', &c., we invent a cause A, then we must in some degree appeal to experience as to the mode in which A will act. As the objects and laws of nature are certainly not known to the mind intuitively, we must point out some other cause B, which supplies the requisite notions, and all we do is to invent a fourth term to an analogy. As B is to its effects b, b', b'', &c., so is A to its effects a, a', a'', &c. When, for instance, we attempt to explain the passage of light and heat radiations through space unoccupied by matter, we imagine the existence of the so-called *ether*. But if this ether were wholly different from anything else known to us, we should in vain try to reason about it. We must at least apply to it the laws of motion, that is, we must

so far liken it to matter. And as when applying those laws to the elastic medium air, we are able to infer the phenomena of sound, so by arguing in a similar manner concerning ether we are able to infer the existence of light phenomena corresponding to what do occur. All that we do is to take a material elastic substance, increase its elasticity in an almost indefinite degree, and denude it of gravity and some others of the ordinary properties of matter, but we must retain sufficient likeness to matter to allow of deductive calculations.

The force of gravity is in some respects an almost incomprehensible existence, but in other respects entirely conformable to experience. We can distinctly observe that the force is proportional to mass, and that it acts in entire independence of the other matter which may be present or intervening. The law of the decrease of intensity as the square of the distance increases, may be observed to hold true of light, sound, and any other influences emanating from a point, and spreading uniformly through space. The law is doubtless connected at this point with the primary properties of space itself, and is so far conformable to our necessary ideas.

It may well be said, however, that no hypothesis can be so much as framed in the mind unless it be more or less conformable to experience. As the material of our ideas is undoubtedly derived from sensation, so we cannot figure to ourselves any existence or agent, but as endowed with some of the properties of matter. All that the mind can do in the creation of new existences is to alter combinations, or by analogy to alter the intensity of sensuous properties. The phenomenon of motion is familiar to sight and touch, and different degrees of rapidity are also familiar: we can pass beyond the limits of sense, and suppose the existence of rapid motion, such as our senses could not measure or observe. We know what is elasticity,

and we can therefore in a certain sense figure to ourselves elasticity a thousand or a million times greater than any which is sensuously known to us. The waves of the ocean are many times higher than our own bodies; other waves, we may observe, are many times less; continue the proportion, and we may ultimately arrive at waves as small as those of light. Thus it is that from a sensuous basis the powers of mind enable us to reason concerning agents and phenomena different in an unlimited degree. If no hypothesis then can be absolutely opposed to sense, accordance with experience must always be a question of degree.

In order that an hypothesis may allow of satisfactory comparison with experience, it must possess a certain definiteness, and, generally speaking, a certain mathematical exactness allowing of the precise calculation of results. We must be able to ascertain whether it does or does not agree with facts.

The theory of vortices, on the contrary, did not present any mode of calculating the exact relations between the distances and periods of the planets and satellites; it could not, therefore, undergo that rigorous testing to which Newton scrupulously submitted his theory of gravity before its promulgation. Vagueness and incapability of precise proof or disproof often enables a false theory to live; but with those who love truth, such vagueness should excite the highest suspicion. The upholders of the ancient doctrine of Nature's abhorrence of a vacuum, had been unable to anticipate the important fact that water would not rise more than 33 feet in a common suction pump. Nor when the fact was pointed out could they explain it, except by introducing a special alteration of the theory to the effect that Nature's abhorrence of a vacuum was limited to 33 feet.

The Second Requisite—Consistency with established Laws of Nature.

In the second place an hypothesis must not be contradictory to what we believe to be true concerning Nature. It must not involve self-inconsistency which is opposed to the highest and simplest laws, namely, those of Logic. Neither ought it to be irreconcileable with the simple laws of motion, of gravity, of the conservation of energy, or any parts of physical science which we consider to be established beyond reasonable doubt. Not that we are absolutely forbidden to adopt such an hypothesis, but if we do so we must be prepared to disprove some of the best demonstrated truths in the possession of mankind. The fact that conflict exists means that the consequences of the theory are not verified if previous discoveries are correct, and we must therefore show that previous discoveries are incorrect before we can verify our theory.

An hypothesis will be exceedingly improbable, not to say invalid, if it supposes a substance or agent to act in a manner unknown in other cases; for it then fails to be verified in our knowledge of that substance or agent. Several physicists, especially Euler and Grove, have supposed that we might dispense with any ethereal basis of light, and infer from the interstellar passage of rays that there was some kind of rare gas occupying space. But if so, that gas must be excessively rare, as we may infer from the apparent absence of an atmosphere around the moon, and from many other facts and laws known to us concerning gases and the atmosphere; and yet at the same time it must possess an elastic force at least a billion times as great as atmospheric air at the earth's surface, in order to account for the extreme rapidity of the light

rays. Such an hypothesis then is inconsistent with the main body of our knowledge concerning gases.

Provided that there be no clear and absolute conflict with known laws of nature, there is nothing so improbable or apparently inconceivable that it may not be rendered highly probable, or even approximately certain, by a sufficient number of concordances. In fact the two best founded and most conspicuously successful theories in the whole range of physical science involve the most absurd suppositions. Gravity is a force which appears to act between bodies through vacuous space; it is in positive contradiction to the old dictum that nothing could act but through some intervening medium or substance. It is even more puzzling that the force acts in perfect indifference to all intervening obstacles. Light in spite of its extreme velocity, shows much respect to matter, for it is almost instantaneously stopped by opaque substances, and to a considerable extent absorbed and deflected by transparent ones. But to gravity all media are, as it were, absolutely transparent, nay non-existent; and two particles at opposite points of the earth affect each other exactly as if the globe were not between. To complete the apparent impossibility, the action is, so far as we can observe, absolutely instantaneous, so that every particle of the universe is at every moment in separate cognizance, as it were, of the relative position of every other particle throughout the universe at that same moment of absolute time. Compared with such incomprehensible conditions, the theory of vortices deals with common-place realities. Newton's celebrated saying, *hypotheses non fingo*, bears the appearance of pure irony; and it was not without apparent grounds that Leibnitz and the greatest continental philosophers charged Newton with re-introducing occult powers and qualities.

The undulatory theory of light presents almost equal

difficulties of conception. We are asked by physical philosophers to give up all our ordinary prepossessions, and believe that the interstellar space which seemed so empty is not empty at all, but filled with *something* immensely more solid and elastic than steel. As Dr. Young himself remarked[f], 'the luminiferous ether, pervading all space, and penetrating almost all substances, is not only highly elastic, but absolutely solid!!!' Sir John Herschel has calculated the amount of force which may be supposed, according to the undulatory theory of light, to be exerted at each point in space, and finds it to be 1,148,000,000,000 times the elastic force of ordinary air at the earth's surface, so that the pressure of the ether upon a square inch of surface must be about 17,000,000,000,000, or seventeen billions of pounds[g]. Yet we live and move without appreciable resistance through this medium, indefinitely harder and more elastic than adamant. All our ordinary notions must be laid aside in contemplating such an hypothesis; yet they are no more than the observed phenomena of light and heat force us to accept. We cannot deny even the strange suggestion of Dr. Young, that there may be independent worlds, some possibly existing in different parts of space, but others perhaps pervading each other unseen and unknown in the same space[h]. For if we are bound to admit the conception of this adamantine firmament, it is equally easy to admit a plurality of such. We see, then, that mere difficulties of conception must not in the least discredit a theory which otherwise agrees with facts, and we must only reject hypotheses which are inconceivable in the sense of breaking distinctly the primary laws of thought and nature.

[f] Young's 'Works,' vol. i. p. 415.

[g] 'Familiar Lectures on Scientific Subjects,' p. 282.

[h] Young's 'Works,' vol. i. p. 417.

The Third Requisite—Conformity with Facts.

Before we accept a new hypothesis, it must furnish us with distinct credentials, consisting in the deductive anticipation of a series of facts, which are not already connected and accounted for by any equally probable hypothesis. We cannot lay down any precise rule as to the number of accordances which can establish the truth of an hypothesis, because the accordances will vary much in value. While, on the one hand, no finite number of accordances will give entire certainty, the probability of the hypothesis will increase very rapidly with the number of accordances. Seldom, indeed, shall we have a theory free from difficulties and apparent inconsistency with facts. Though one real and undoubted inconsistency would be sufficient to overturn the most plausible theory, yet there is usually some probability that the fact may be misinterpreted, or that some supposed law of nature, on which we are relying, may not be true. Almost every problem in science thus takes the form of a balance of probabilities. It is only when difficulty after difficulty has been successfully explained away, and decisive *experimenta crucis* have, time after time, resulted in favour of our theory, that we can venture to assert the falsity of all objections.

The sole real test of an hypothesis is its accordance with fact. Descartes' celebrated system of vortices is exploded and rejected, not because it was intrinsically absurd and inconceivable, but because it could not give results in accordance with the actual motions of the heavenly bodies. The difficulties of conception involved in the apparatus of vortices, are mere child's play compared with those of gravitation and the undulatory theory already described. The vortices are on the whole plausible suppositions; for the planets and satellites bear at first sight much resemblance to objects carried round in whirlpools, an

analogy which doubtless suggested the theory. The failure was in the first and third requisites; for, as already remarked, the theory did not allow of any precise calculation of planetary motions, and was so far incapable of rigorous verification. But so far as we can institute a comparison, facts are entirely against the vortices. Newton carefully pointed out that the Cartesian theory was inconsistent with the laws of Kepler, and would represent the planets as moving more rapidly at their aphelia than at their perihelia[i]. Newton did not ridicule the theory as absurd, but showed[k] that it was 'pressed with many difficulties.' The rotatory motions of the sun and planets on their own axes are in striking conflict with the revolutions of the satellites carried round them; and comets, the most flimsy of bodies, calmly pursue their courses in elliptic paths, altogether irrespective of the vortices which they intersect. We may now also point to the interlacing orbits of the minor planets as a new and insuperable difficulty in the way of the Cartesian ideas.

Newton, though he established the best of theories, was also capable of proposing one of the worst; and if we want an instance of a theory decisively contradicted by facts, we have only to turn to his views concerning the origin of natural colours. Having analysed, with incomparable skill, the origin of the colours of thin plates, he suggests that the colours of all bodies and substances are determined in like manner by the size of their ultimate particles. A thin plate of a definite thickness will reflect a definite colour; hence, if broken up into fragments it will form a powder of the same colour. But, if this be a sufficient explanation of coloured substances, then every coloured fluid ought to reflect the complementary colour of that which it transmits. Colourless transparency arises,

[i] 'Principia,' bk. II. Sect. ix. Prop. 53.

[k] Ibid. bk. III. Prop. 43. General Scholium.

according to Newton, from all the particles being too minute to reflect light; but if so, every transparent substance should appear perfectly black by reflected light, and, *vice versâ*, every black substance should be transparent. Newton himself so acutely felt this last difficulty as to suggest that true blackness is due to some internal refraction of the rays to and fro, and an ultimate stifling of them, which he did not attempt further to explain. Unless some other process came into operation, neither refraction nor reflection, however often repeated, would destroy the energy of light. The theory gives no account, therefore, as Brewster shows, of 24 parts out of 25 of the light which falls upon a black coal, and the $\frac{1}{25}$th part which is reflected from the lustrous surface is equally inconsistent with the theory, because fine coal-dust is almost entirely devoid of reflective power[1]. It is now generally believed that the colours of natural bodies are due to the unequal absorption of rays of light of different refrangibility.

Experimentum Crucis.

As we deduce more and more conclusions from a theory, and find them verified by trial, the probability of the theory increases in a most rapid manner; but we never escape the risk of error altogether. Absolute certainty is beyond the power of inductive investigation, and the most plausible suppositions may ultimately be proved false. Such is the groundwork of similarity in nature, that two very different conditions may often give closely similar results. We sometimes find ourselves therefore in possession of two or more hypotheses which both agree

[1] Brewster's 'Life of Newton,' 1st edit. chap. vii.

with so many experimental facts as to have great appearance of truth. Under such circumstances we have need of some new experiment, which shall give results agreeing with one hypothesis but not with the other.

Any such experiment which decides between two rival theories may be called an *Experimentum Crucis*, an Experiment of the Finger Post. Whenever the mind stands, as it were, at cross-roads, and knows not which way to select, it needs some decisive guide, and Bacon therefore assigned great importance and authority to instances or facts which serve in this capacity. The name given by Bacon has become exceedingly familiar; it is perhaps almost the only one of Bacon's figurative expressions which has passed into common use. We even find Newton, as I have already mentioned, using the name (vol. ii. p. 134).

I do not think, indeed, that the common use of the word at all agrees with that intended by Bacon. Sir John Herschel says that 'we make an experiment of the crucial kind when we form combinations, and put in action causes from which some particular one shall be deliberately excluded, and some other purposely admitted[m].' This, however, seems to be the description of any special experiment not made at haphazard. Pascal's experiment of causing a barometer to be carried to the top of the Puy-de-Dôme has often been considered as a perfect *experimentum crucis*, if not the first distinct one on record[n]; but if so, we must dignify the doctrine of Nature's abhorrence of a vacuum with the position of a rival theory. A crucial experiment must not simply confirm one theory, but must negative another; it must decide a mind which is in equilibrium, as Bacon says[o],

[m] 'Discourse on the Study of Natural Philosophy,' p. 151.

[n] Ibid. p. 229.

[o] 'Novum Organum,' bk. II. Aphorism 36.

between two equally plausible views. 'When in search of any nature, the understanding comes to an equilibrium, as it were, or stands suspended as to which of two or more natures the cause of nature inquired after should be attributed or assigned, by reason of the frequent and common occurrence of several natures, then these Crucial Instances show the true and inviolable association of one of these natures to the nature sought, and the uncertain and separable alliance of the other, whereby the question is decided, the former nature admitted for the cause, and the other rejected. These instances, therefore, afford great light, and have a kind of overruling authority, so that the course of interpretation will sometimes terminate in them, or be finished by them.'

The long continued strife between the Corpuscular and Undulatory theories of light forms the best possible illustration of the need of an Experimentum Crucis. It is highly remarkable in how complete and plausible a manner both these theories agreed with the ordinary laws of geometrical optics, relating to reflection and refraction.

A moving particle, according to the first law of motion, proceeds in a perfectly straight line, when undisturbed by extraneous forces. If the particle, being perfectly elastic, strike a perfectly elastic plane, it will bound off in such a path that the angles of incidence and reflection will be equal. Now a ray of light proceeds in a perfectly straight line, or appears to do so, until it meets a reflecting body, when its path is altered in a manner exactly similar to that of the elastic particle. Here is a remarkable correspondence which probably suggested to Newton's mind that light consisted of minute elastic particles moving with excessive rapidity in straight lines. The correspondence was found to extend also to the law of simple refraction; for if these particles of light be supposed capable of attracting matter, and being attracted by it at insensibly small distances,

then a ray of light, falling on the surface of a transparent medium, will suffer an increase in its velocity of motion perpendicular to the surface, and the familiar law of sines is the necessary consequence. This remarkable explanation of the law of refraction had doubtless a very strong effect in leading Newton to entertain the corpuscular theory, and he appears to have thought that the analogy between the propagation of the rays of light and the motion of bodies was perfectly exact, whatever might be the actual nature of light[p]. It is highly remarkable, again, that Newton was able to give, by his corpuscular theory, a plausible explanation of the inflection of light as discovered by Grimaldi. The theory would indeed have been a very probable one could Newton's own law of gravity have been applied; but this was excluded, because the particles of light, in order that they may move in straight lines, must be assumed devoid of any influence upon each other.

The Huyghenian or Undulatory theory of light was also able to explain the same phenomena, but with one remarkable difference. If the undulatory theory be true, light must move more slowly in a dense refracting medium than in a rarer one; but the Newtonian theory assumed that the attraction of the dense medium caused the particles of light to move more rapidly than in the rare medium. On this point, then, there was a complete discrepancy between the two theories, and observation was required to show which theory was to be preferred. Now by simply cutting a uniform plate of glass into two pieces, and slightly inclining one piece so as to increase the length of the path of a ray passing through it, experimenters have been able to show that the light does move

[p] 'Principia,' bk. I. Sect. xiv. Prop. 96. Scholium, 'Opticks,' Prop. VI. 3rd edit. p. 70.

more slowly in glass than in air[q]. More recently, in 1850, Fizeau and Foucault independently measured the velocity of light in air and water by a revolving mirror, and found that the velocity is greater in air[r]. There are indeed a number of other points at which experience decides against Newton, and in favour of Huyghens and Young. Euler rejected the Corpuscular theory because particles of matter moving with the immense velocity of light must possess great momentum, of which there is no evidence in fact[s]. Bennet concentrated the light and heat of the sun upon a body so delicately suspended that an exceedingly small amount of momentum must have been rendered apparent, but there was no such effect[t]. This experiment, indeed, is of a negative kind, and is not absolutely conclusive, unless we could estimate the momentum which Newton's theory would require to be present (see vol. ii. p. 45); but there are other difficulties. Laplace pointed out that the attraction supposed to exist between matter and the corpuscular particles of light, would cause the velocity of light to vary with the size of the emitting body, so that if a star were 250 times as great in diameter as our sun, its attraction would prevent the emanation of light altogether[u]. But so far as experience shows, the velocity of light is uniform, and independent of the magnitude of the emitting body, as it should be according to the undulatory theory. Lastly, Newton's explanation of diffraction or inflection fringes of colours was only *plausible*, and not true; for Fresnel ascertained that the dimensions of the fringes are not what they would be according to Newton's theory.

[q] Airy's 'Mathematical Tracts,' 3rd edit. pp. 286–288.

[r] Jamin, 'Cours de Physique,' vol. iii. p. 372.

[s] Euler's 'Letters,' vol. ii. Letter XIX. p. 69.

[t] Balfour Stewart, 'Elementary Treatise on Heat,' p. 161.

[u] Young's 'Lectures on Natural Philosophy' (1845), vol. i. p. 361.

Although the Science of Light presents us with the most beautiful examples of crucial experiments and observations, instances are not wanting in other branches of science. Copernicus asserted in opposition to the ancient Ptolemaic theory that the earth and planets moved round the sun, and he predicted that if ever the sense of sight could be rendered sufficiently acute and powerful, we should see phases in Mercury and Venus. Galileo with his telescope was able, in 1610, to verify the prediction as regards Venus, and subsequent observations of Mercury lead to a like conclusion. The discovery of the aberration of light added a new proof, still further strengthened by the more recent determination of the parallax of fixed stars. Hooke proposed to prove the existence of the earth's diurnal motion by observing the deviation of a falling body, an experiment successfully accomplished by Benzenberg; and Foucault's pendulum has since furnished an additional indication of the same motion, which is indeed also apparent in the direction of the trade winds. All these are crucial facts in favour of the Copernican theory.

Davy's discovery of potassium and sodium in 1807 was a good instance of a crucial experiment; for it decisively confirmed Lavoisier's views, and at the same time negatived the ancient notions of phlogiston.

Descriptive Hypotheses.

There are some, or probably many, hypotheses which we may call *descriptive hypotheses*, and which serve for little else than to furnish convenient names. When a certain phenomenon is of an unusual and mysterious kind, we cannot even speak of it without using some analogy. Every word implies some resemblance between the thing to which it is applied, and some other thing, which fixes

the meaning of the word. Thus if we are to speak of what constitutes electricity, we must search for the nearest analogy, and as electricity is chiefly characterised by the rapidity and facility of its movements, the notion of a fluid of a very subtle character presented itself as most appropriate. There is the single fluid and the double fluid theory of electricity, and a great deal of discussion has been uselessly spent upon them. The fact is that if these theories be understood as more than convenient modes of describing the phenomena, they are grossly invalid. The analogy extends only to the rapidity of motion, and the fact that a phenomenon occurs successively at different points of the body. The so-called electric fluid adds nothing to the weight of the conductor, and to suppose that it really consists of particles of matter would be even more absurd than to reinstate the Corpuscular theory of light. An infinitely closer analogy exists between electricity and light undulations, which are about equally rapid in propagation; and while we shall probably continue for a long time to talk of the electric fluid, there can be no doubt that this expression merely represents some phase of molecular motion, some wave of disturbance propagating itself at one time through material conductors, at another time through the ethereal basis of light. The invalidity of these fluid theories is moreover shown in the fact that they have not led to the invention of a single new experiment. When we speak of heat as *flowing* from one body to another, we likewise use a descriptive hypothesis merely; for Lambert's theory of the fluid motion of heat is no better than the Corpuscular theory of light.

Among these merely descriptive hypotheses I should be inclined to place Newton's theory of Fits of Easy Reflection and Refraction. That theory has been since exploded by actual discordance with fact, but even when

really entertained it did not do more than describe what took place. It involved no deep analogy to any other phenomena of nature, for Newton could not point to any other substance which went through these extraordinary changes. We now know that the true analogy would have been the waves of sound, of which Newton had acquired in other respects so complete a comprehension. But though the notion of interference of waves had distinctly occurred to Hooke, Newton had failed to see how the periodic phenomena of light could be connected with the periodic character of waves. His hypothesis fell because it was out of analogy with everything else in nature, and it therefore did not allow him, as in other cases, to descend by mathematical deduction to consequences which could be verified or refuted.

We are always at freedom again to imagine the existence of a new agent or force, and give it an appropriate name, provided there are phenomena incapable of explanation from known causes. We may speak of *vital force* as occasioning life, provided that we do not take it to be more than a name for an undefined something giving rise to inexplicable facts, just as the French chemists called Iodine the Substance X, while they were unaware of its real character and place in chemistry[y]. Encke was quite justified in speaking of the *resisting medium* in space so long as the retardation of his comet could not be otherwise accounted for. But such hypotheses will do much harm whenever they divert us from attempts to reconcile the facts with known laws, or when they lead us to mix up entirely discrete things. We have no right, for instance, to confuse Encke's supposed resisting medium with the ethereal basis of light. The name protoplasm, now so familiarly used by physiologists, is doubtless legitimate so long as we do not mix up different sub-

[y] Paris, 'Life of Davy,' p. 274.

stances under it, or imagine that the name gives us any knowledge of the obscure origin of life. To name a substance protoplasm no more explains the infinite variety of forms of life which spring out of the substance, than does the *vital force* which may be supposed to reside in the protoplasm. Both expressions appear to me to be mere names for an unknown and inexplicable series of causes which out of apparently similar conditions produce the most diverse results.

Hardly to be distinguished from descriptive hypotheses are certain imaginary objects or conditions which we often frame for the more ready investigation or comprehension of a subject. The mathematician, in treating abstract questions of probability, finds it convenient, to represent the conditions to his own or other minds by a concrete analogy in the shape of a material ballot-box. The fundamental principle of the inverse method of probabilities upon which depends the whole of our reasoning in inductive investigations is proved by Poisson, who imagines a number of ballot-boxes, of which the contents are afterwards supposed to be mixed in one great box (vol. i. p. 280). Many other such devices are also used by mathematicians. When Newton investigated the nature of waves, he employed the pendulum as a convenient mode of representing the nature of the undulation. Centres of gravity, oscillation, &c., poles of the magnet, lines of force, are other imaginary existences solely employed to assist our thoughts (vol. i. p. 422). All such creations of the mind may be called *Representative Hypotheses*, and they are only permissible and useful so far as they embody analogies. Their further consideration properly belongs either to the subject of Analogy, or to that of language and representation, founded upon analogy.

CHAPTER XXIV.

EMPIRICAL KNOWLEDGE, EXPLANATION, AND PREDICTION.

THE one great method of inductive investigation, as we have seen, consists in the union of hypothesis and experiment, deductive reasoning being the link by which the experimental results are made to confirm or confute the hypothesis. Now when we consider this relation between hypothesis and experiment, it is obvious that we may classify our knowledge under four heads.

(1) We may be acquainted with facts or phenomena which have come under our notice accidentally or without reference to any special hypothesis, and which have not been brought into accordance as yet with any hypothesis. Such facts constitute what is called *Empirical Knowledge.*

(2) Another very extensive portion of our knowledge consists of those facts which, having been first observed empirically, have afterwards been brought into accordance with other facts by an hypothesis concerning the general laws applying to them. This portion of our knowledge may be said to be *explained, reasoned,* or *generalised.*

(3) In a third place comes the collection of facts, minor in number, but most important as regards their scientific value and interest, which have been anticipated by theory and afterwards verified by experiment.

(4) Lastly, there may and does exist knowledge of phenomena accepted solely on the ground of theory, and which is incapable of experimental confirmation, at least with the instrumental means at the time in our possession.

It is a work of much interest to compare and illustrate in some degree the relative extent and value of these four groups of knowledge. As a general rule we shall observe that every great branch of science originates in facts observed accidentally, or without any distinct consciousness of what is to be expected. But as science progresses, its power of foresight rapidly increases, until the mathematician in his study seems to acquire the power of anticipating nature, and predicting what will happen in stated circumstances before the eye of man has ever witnessed the event.

Empirical Knowledge.

By empirical knowledge we mean such as is derived directly from the examination of certain detached facts, and rests entirely on those facts, without corroboration or connexion with other branches of knowledge. It is contrasted to generalised and theoretical knowledge, which embraces many series of facts under a few simple and comprehensive principles, so that each series serves to throw light upon each other series of facts. Just as, in the map of a half-explored country, we see detached portions of rivers, isolated mountains, and undefined plains, not connected into any general plan, so a new branch of knowledge often consists of groups of facts, each group standing apart, so as not to allow us to reason from one part to another.

Before the time of Descartes, and Newton, and Huyghens, there was much empirical knowledge of the

phenomena of light. The rainbow had always struck the attention of the most careless observers, and there was no difficulty in perceiving that its conditions of occurrence consisted in rays of the sun shining upon falling drops of rain. It was impossible to overlook the resemblance of the ordinary rainbow to the comparatively rare lunar rainbow, to the bow which often appears upon the spray of a waterfall, or even upon beads of dew suspended on grass and spiders' webs. In all these cases the uniform conditions are rays of light and round drops of water. Roger Bacon had noticed these conditions, as well as the analogy of the rainbow colours to those produced by crystals[a]. But the knowledge was empirical until Descartes and Newton showed how the phenomena were connected with all the other facts concerning the refraction of light.

There can be no better instance of an empirical truth than that detected by Newton concerning the high refractive powers of combustible substances. Newton's chemical notions were almost as vague as those prevalent in his day, but he observed that certain 'fat, sulphureous, unctuous bodies,' as he calls them, such as camphor, oils, spirit of turpentine, amber, &c., have refractive powers two or three times greater than might be anticipated from their densities[b]. The enormous refractive index of diamond, led him with great sagacity to regard it as also of the same unctuous or inflammable nature, so that he may be regarded as predicting the combustibility of the diamond, afterwards demonstrated by the Florentine Academicians in 1694. Brewster having entered into a long investigation of the refractive powers of different substances, confirmed Newton's assertions, and found that

[a] 'Opus Majus.' Edit. 1733. Cap. x. p. 460.

[b] Newton's 'Opticks.' Third edit. p. 249. Leslie's 'Dissertation,' Encyclopædia Britannica, p. 550.

the three elementary combustible substances, diamond, phosphorus, and sulphur, have by far the highest refractive indices known in proportion to their densities[c], and there are only a few substances, such as chromate of lead or glass of antimony, known to exceed them in absolute power of refraction. The oils and hydrocarbons generally possess an excessive index. But this knowledge remains to the present day purely empirical, no connexion having been pointed out between this coincidence of inflammability and high refractive power, with other laws of chemistry or optics. It is worthy of notice, however, as pointed out by Brewster, that if Newton had argued concerning two minerals, Greenockite and Octahedrite, as he did concerning diamond, his predictions would have proved false. In the present day, the relation of the refractive index to the density and atomic weight of a substance is becoming a matter of theory; yet there remain specific differences of refractive power known only on empirical grounds, and it is curious that in hydrogen also an abnormally high refractive power has been found to be joined to inflammability.

The science of chemistry, however much its theory may have progressed, still presents us with a vast body of empirical knowledge. Not only is it at present hopeless to attempt to account for the particular group of qualities belonging to each element, but there are multitudes of particular facts of which no further account can be given. Why should the sulphides of many metals be intensely black? Why should a slight amount of phosphoric acid have so great a power of interference with the crystallization of vanadic acid[d]. Why should the compound silicates of alkalies and alkaline metals be transparent? Why should gold be so highly ductile, and gold and silver the

[c] Brewster, 'Treatise on New Philosophical Instruments,' p. 266, &c.

[d] Roscoe, Bakerian Lecture, 'Philosophical Trans.' (1868), vol. clviii. p. 6.

only two sensibly translucent metals? Why should sulphur be capable of so many peculiar changes into amorphous conditions?

There are whole branches of chemical knowledge which are as yet mere aggregates of disconnected facts. The properties of alloys, or mixtures of metals, are often exceedingly remarkable; but no laws have yet been detected, and the usual laws of combining proportions seem to have no clear application[e]. Not the slightest explanation can be given of the wonderful variations of the qualities of iron, according as it contains more or less carbon and silicon, nay, even the facts of the case are often involved in uncertainty. Why, again, should the properties of steel be remarkably affected by the presence of a little tungsten. All that was determined by Matthiessen concerning the variation of the conducting powers of copper according to its purity, was of a purely empirical character[f]. Many animal substances cannot be shown to obey even the laws of combining proportions. Thus for the most part chemistry is yet a science occupied with an exact description of artificial or natural substances, which by the collection of enormous numbers of exact facts is preparing the way for an extension of theory at some future time.

We must not indeed suppose that any science will ever entirely cease to be empirical. Multitudes of phenomena have been explained by the undulatory theory of light; but there remains an almost undiminished mass of facts yet to be treated. The natural colours of bodies, and the rays given off by them when heated, are yet free from all theory, and yield few empirical coincidences. The theory of electricity is partially understood, but the conditions of the production of frictional electricity defy law or ex-

[e] 'Life of Faraday,' vol. ii. p. 104.

[f] Watts, 'Dictionary of Chemistry,' vol. ii. p. 39, &c.

planation, although they have been studied for two centuries or more. I shall subsequently point out that even the establishment of a wide and true law of nature is but the starting-point for the discovery of exceptions or slight divergences giving a wide scope to empirical discovery.

There is probably no science, I have said, which is entirely free from empirical and unexplained facts. Logic approaches most nearly to this position, as it is merely a deductive development of the laws of thought and the principles of substitution. Yet some of the facts established in the investigation of the inverse logical problem (vol. i. p. 157) may be considered empirical. Mathematical science often yields empirical truths. Why, for instance, should the value of π, when expressed to a great number of figures, contain the digit 7 much less frequently than any other digit[g]? Even geometry may allow of empirical truths, when the matter does not involve quantities of space, but numerical results and the positive or negative character of quantities, as in De Morgan's theorem concerning negative areas.

Accidental Discovery.

There are not a few cases where almost pure accident has undoubtedly determined the moment when a new branch of knowledge was to be created. The true laws of the construction of crystals were not discovered until Haüy happened to drop a beautiful crystal of calc-spar upon a stone pavement. His momentary regret, at destroying a choice specimen, was quickly removed when, in attempting to join the fragments together, he observed regular geometrical faces, which did not correspond with the external facets of the crystals. A great many more crystals were soon broken intentionally, to observe the

g De Morgan's 'Budget of Paradoxes,' p. 291.

planes of cleavage, and a nearly complete comprehension of the internal structure of crystalline substances was soon the result. Here we see how much more was due to the reasoning powers of the philosopher, than to an accident which must often have happened to other persons.

In a similar manner, a purely fortuitous occurrence led Malus to discover the polarization of light by reflection. The phenomena of double refraction had, of course, been long known, and when engaged in Paris in 1808, in investigating the character of light thus polarized, Malus chanced to look through a double refracting prism at the light of the setting sun, reflected from the windows of the Luxembourg Palace. In turning the prism round, he was surprised to find that the ordinary image disappeared at two opposite positions of the prism. He remarked that the reflected light behaved exactly like light which had been already polarized by passing through another prism. He was induced to test the character of light reflected under other circumstances, and it was eventually proved that polarization is connected by invariable laws with the act of reflection. Some of the most general laws of optics, previously unsuspected, were thus discovered by pure accident.

In the history of electricity, accident has had a large part. For centuries some of the more common effects of magnetism, or frictional electricity, had presented themselves as exceptional and unaccountable deviations from the ordinary course of Nature. Accident must, of course, have first directed attention to such phenomena, but how few of those who witnessed them had any conception of the all-pervading power thus manifested. The very existence of the so-called galvanism, or electricity of low tension, was unsuspected until Galvani accidentally touched the leg of a frog with pieces of metal. The decomposition of water by voltaic electricity is also said to have been accidentally discovered by Nicholson in 1801,

and Davy speaks of this discovery as the foundation of all that had since been done in electro-chemical science.

It is otherwise with the discovery of electro-magnetism, or the relation between the magnet and electricity. Oersted, in common with many others, had suspected the existence of some relation between these strange powers, and he appears to have tried to detect its exact nature. Once, as we are told by Hansteen, he had employed a strong galvanic battery during a lecture, and at the close it occurred to him to try the effect of placing the conducting wire parallel to a magnetic needle, instead of at right angles, as he had previously done. The needle immediately moved and took up a position nearly at right angles to the wire; he inverted the direction of the current, and the needle deviated in a contrary direction. The great discovery was made, and if by accident, it was such an accident as happens only to those who deserve them, as Lagrange remarked of Newton [h]. There was, in fact, nothing accidental, except that, as in all totally new discoveries, Oersted did not know what to look for. He could not infer from previous knowledge the nature of the relation, and it was only repeated trial in different modes which could lead him to the right combination. High and happy powers of inference, and not accident, subsequently induced Faraday to reverse the process, and show that the motion of the magnet would occasion an electric current in the wire.

Sufficient investigation would probably show that almost every branch of art and science had an accidental beginning. In historical times almost every important new instrument, such as the telescope, the microscope, or the compass, was probably suggested by some accidental occurrence or observation. In pre-historic times the germs of the arts must have arisen still more exclusively in

[h] 'Life of Faraday,' vol. ii. p. 396.

the same way. Cultivation of plants probably arose, in Mr. Darwin's opinion, from some such accident as the seeds of a fruit falling upon a heap of refuse, and producing an unusually fine variety. Even the use of fire must, some time or other, have been discovered in a like accidental manner.

With the progress of any branch of science, the element of chance becomes much reduced. Not only are laws discovered which enable results to be predicted, as we shall shortly consider, but the systematic examination of phenomena and substances often leads to important and novel discoveries, which can in no true sense be said to be accidental. It has been asserted that the anæsthetic properties of chloroform were disclosed by a little dog smelling at a saucerful of the liquid in a chemist's shop in Linlithgow, the singular effects upon the dog being reported to Dr. Simpson, who turned the incident to such good account. This story, however, has since been shown to be a fabrication, the fact being that Dr. Simpson had for many years being endeavouring to discover a better anæsthetic than those previously employed, and that he tested the properties of chloroform, among other substances, at the suggestion of Mr. Waldie, a Liverpool chemist. The valuble powers of hydrate of chloral have since been discovered in a like manner, and systematic inquiries are continually being made into the therapeutic or economic value of new chemical compounds.

If we must attempt to draw any conclusion concerning the part which chance plays in scientific discovery, it must be allowed that it more or less affects the success of all inductive investigation, but becomes less important with the progress of any particular branch of science. Accident, too, may bring a new and valuable combination to the notice of some person who had never expressly searched for a discovery of the kind, and the probabilities

are certainly in favour of a discovery being occasionally made in this manner. But the greater the tact and industry with which a physicist applies himself to the study of nature, the greater is the probability that he will meet with fortunate accidents, and will turn them to good account. Thus it comes to pass that, in the refined investigations of the present day, genius united to extensive knowledge, cultivated powers and indomitable industry, constitute the characteristics of the great discoverer.

Empirical Observations subsequently Explained.

The second great portion of scientific knowledge consists of facts which have been first learnt in a purely empirical manner, but have afterwards been shown to follow from some law of nature, that is, from some highly probable hypothesis. Facts are said to be explained when they are thus brought into harmony with other facts, or bodies of general knowledge. There are few words more familiarly used in scientific phraseology than this word *explanation*, and it is necessary to decide exactly what we mean by it, since the question touches the very deepest points concerning the nature of science. Like most terms referring to mental actions, the verbs *to explain*, or *to explicate*, involve material similes. The action is *ex plicis plana reddere*, to take out the folds, and render a thing plain or even. Explanation thus renders a thing clearly comprehensible in all its points, so that there is nothing left outstanding or obscure.

Every act of explanation consists in detecting and pointing out a resemblance between facts, or in showing that a greater or less degree of identity exists between apparently diverse phenomena. This resemblance may be of any extent and depth; it may be a general law of

nature, which explains and harmonizes the motions of all the heavenly bodies, that is, shows that there is a similar force which governs all those motions, or the explanation may involve nothing more than a single identity, as when we explain the appearance of shooting stars by showing that they are identical with portions of a comet. Wherever we detect resemblance, there is a more or less satisfactory explanation. The mind is always somewhat disquieted when it meets a novel phenomena, one which is *sui generis;* it seeks at once for any parallels which may be found in the memory of past sensations. The so-called sulphurous smell which attends a stroke of lightning long excited the attention and fears of men, and it was not explained, until the exact similarity of the smell to that of ozone, or allotropic oxygen, was pointed out. The marks upon a flagstone are explained when they are shown to correspond with the feet of an extinct animal, whose bones are elsewhere found. Explanation, in fact, generally commences by the discovery of some very simple resemblance; the theory of the rainbow began as soon as Antonio de Dominis pointed out the resemblance between its colours and those presented by a ray of sunlight passing through a glass globe full of water.

The nature and limits of explanation can only be fully considered, after we have entered upon the subject of generalization and analogy. It must suffice to remark, in this place, that the most important process of explanation consists in showing that an observed fact is only one case of a general law or tendency. Iron is always found combined with sulphur, when it is in contact with or included in coal, whereas in other parts of the coal strata it always occurs as a carbonate. We explain this empirical fact as being due to the ordinary reducing powers of carbon and hydrogen, which prevent the iron from combining with oxygen, and leave it open to the affinity of the sulphur.

The uniform direction and strength of the trade-winds were long familiar to mariners, before they were explained by Halley on hydrostatical principles. The winds were found to arise from the action of gravity, which causes any heavy body to displace a lighter one, while the direction from east to west was also explained as a necessary result of the earth's rotation. Whatever body moves in the northern hemisphere from north to south, whether it be a bird, or a railway train, or a body of air, must tend towards the right hand, or west. Dove's law of the winds is to the effect that the winds tend to veer in the northern hemisphere in the direction N.E.S.W., and in the southern hemisphere in the direction N.W.S.E. This tendency was shown by him to be the necessary effect of the same conditions which apply to the trade-winds. Whenever, then, any fact is connected by resemblance, law, theory, hypothesis, or any other process of reasoning, with other facts, it is explained.

Although the great mass of recorded facts must be empirical, and awaiting explanation, such knowledge is of minor value, because it does not admit of extensive and safe inference. Each recorded result informs us exactly what will be experienced again in the same circumstances, but has no bearing upon what will happen in other circumstances.

Overlooked Results of Theory.

We must by no means suppose that, even when a scientific truth is firmly in our possession, all its consequences will be foreseen. Deduction is, as I have frequently remarked, certain and infallible, in the sense that each step in deductive reasoning will lead us to some result, as certain as the law itself. But it does not follow that every mode of deducing a fact from a law, or a

combination of laws, will occur to a reasoner. Whatever road a traveller takes, he is sure to arrive somewhere, but unless he proceed in a very systematic manner, it is very unlikely that he will reach every place to which a network of roads will conduct him. In like manner there are many phenomena which were virtually within the reach of philosophers by inference from their previous knowledge, but were never discovered until accident or systematic empirical observation disclosed their existence.

That light is propagated with a certain uniform but very high velocity, was proved by Roemer, by observation of the eclipses of Jupiter's satellites. Corrections could henceforward be made in all astronomical observations requiring it, for the difference of absolute time at which an event happens, and that at which it becomes evident to us. But no person happened to remark that the motion of light compounded with that of the earth in its orbit would occasion a small apparent displacement of the greater part of the heavenly bodies. Fifty years elapsed before Bradley empirically discovered this effect, called by him aberration, when examining his accurate observations of the fixed stars [i].

When once the relation between an electric current and a magnet had been detected by Oersted and Faraday, it ought, theoretically speaking, to have been possible for them or any other person to foresee the diverse results which must ensue in different circumstances. If, for instance, a plate of copper were placed beneath an oscillating magnetic needle it should have been seen that the needle would induce currents in the copper, but as this could not take place without a certain reaction against the needle, it ought to have been seen that the needle would come to rest more rapidly than in the absence of the copper. Yet this peculiar effect was accidentally discovered by Gambey

[i] Laplace, 'Précis de l'histoire de l'Astronomie,' p. 104.

in 1824. Arago acutely inferred from Gambey's experiment that if the copper were set in rotation while the needle was stationary the motion would gradually be communicated to the needle. The phenomenon nevertheless puzzled the whole scientific world, and it required the deductive genius of Faraday to show that it was a necessary result of the principles of electro-magnetism[k]. By an act of deductive reasoning Faraday anticipated that a piece of copper rotating between the poles of a powerful magnet must experience a kind of resistance which will soon bring it to rest, and this effect he proved to exist in a decisive experiment[l].

Many other curious facts might be mentioned which when once noticed were explained as the effects of well-known natural laws. It was accidentally discovered that the navigation of canals of small depth could be greatly facilitated by increasing the speed of the boats, the resistance being actually reduced by this increase of speed, which enables the boat to ride as it were upon its own forced wave. Now mathematical theory might have predicted this result had the right application of the formulæ occurred to any one[m]. Giffard's injector for supplying steam boilers with water by the force of their own steam, was, I believe, accidentally discovered, but no new principles of mechanics are involved in it, so that it might have been theoretically invented. The same may be said of the curious experiment in which a stream of air or steam issuing from a pipe is made to hold a free disc upon the end of the pipe and thus apparently obstruct its own free outlet. The possession then of a true theory does not by any means imply the foreseeing of all the

k 'Experimental Researches in Electricity,' 1st Series, pp. 24–44. Paragraphs 81–139.

l Jamin, 'Cours de Physique,' tom. iii. p. 297.

m Airy, 'On Tides and Waves,' Encyclopædia Metropolitana, p. 348 *.

results. The effects of even a few simple laws may be infinitely diverse, and some of the most curious and useful effects may remain undetected until accidental observation brings them to our notice.

Predicted Discoveries.

The most interesting of the four classes of facts or phenomena as specified in p. 157, is probably the third—containing those the occurrence of which has been first predicted by theory, and then verified by observation. There is no more convincing proof of the soundness of scientific knowledge than that it thus confers the gift of foresight. Auguste Comte said that 'Prevision is the test of true theory;' I should say that it is only one test of true theory, but that which is most likely to strike the public attention. Coincidence with fact is the test of true theory, but when the result of theory is announced beforehand, there can be no possible doubt as to the unprejudiced and confident spirit in which the theorist interprets the results of his own theory.

The earliest instance of scientific prophecy is naturally furnished by the science of Astronomy, which was the earliest in development. Herodotus narrates[n] that, in the midst of a battle between the Medes and Lydians, the day was suddenly turned into night, and the event had been foretold by Thales, the Father of Philosophy. A cessation of the combat and a peace confirmed by marriages was the immediate consequence of this happy scientific effort. Much controversy has taken place concerning the exact date of this occurrence, Baily assigning the year 610 B.C., but Sir G. B. Airy has lately decided that the exact day was the 28th of May, 584 B.C.

[n] Lib. i. cap. 74.

There can be no doubt that this and other predictions of eclipses attributed to ancient philosophers were due to an obscure knowledge of the Metonic Cycle, a period of 6585 days, or 223 lunar months, or about 19 years in which a nearly perfect recurrence of the phases and eclipses of the moon takes place; but if so, Thales must have had access to a long series of astronomical records either those of the Egyptians or the Chaldeans. There is a well known story as to the happy use which Columbus made of the power of predicting eclipses in overawing the islanders of Jamaica who refused him necessary supplies of food for his fleet. He threatened to deprive them of the moon's light. 'His threat was treated at first with indifference, but when the eclipse actually commenced, the barbarians vied with each other in the production of the necessary supplies for the Spanish fleet.'

Exactly the same kind of interest and awe which the ancients experienced at the prediction of eclipses, has been felt in modern times concerning the return of comets. Seneca indeed asserted in most distinct and remarkable terms that comets would be found to revolve in periodic orbits and return to sight. The ancient Chaldeans and the Pythagoreans are also said to have entertained a like opinion. But it was not until the age of Newton and Halley that it became possible to calculate the path of a comet in future years. A great comet appeared in 1682, a few years before the first publication of the 'Principia,' and Halley showed that its orbit corresponded with those of remarkable comets rudely recorded to have appeared in the years 1531 and 1607. The intervals of time indeed were not quite equal, but Halley conceived the bold idea that this difference might be due to the disturbing power of Jupiter, near which great planet the comet had passed in the interval 1607–1682. He predicted that the comet would return about the end of 1758 or the beginning of

1759, and though Halley did not live to enjoy the sight, it was actually detected on the night of Christmas-day, 1758. A second return of the comet was witnessed in 1835 nearly at the time anticipated.

In recent times the discovery of Neptune has been the most remarkable instance of prevision in astronomical science. A full account of this discovery may be found in several works, as for instance Herschel's 'Outlines of Astronomy' and 'Grant's History of Physical Astronomy,' Chapters XII and XIII.

Predictions in the Science of Light.

Next after astronomy the science of physical optics has furnished the most beautiful and early instances of the prophetic power of correct theory. These cases are the more striking because they proceed from the profound application of mathematical analysis, and show an insight into the mysterious workings of matter which is surprising to all, but especially to the great majority of men who are unable to comprehend the methods of research employed. By its power of prevision the truth of the undulatory theory of light has been conspicuously proved, and it is especially to be remarked that even Newton received no assistance from his Corpuscular theory in the detection of new experiments. To his followers who embraced that theory we owe little or nothing in the science of light, and even the lofty genius of Laplace did not derive from it a single discovery. As Fresnel himself remarks[o] :—

'The assistance to be derived from a good theory is not to be confined to the calculation of the forces when the laws of the phenomena are known. There are certain laws so complicated and so singular, that observation

[o] Taylor's 'Scientific Memoirs,' vol. v. p. 241.

alone, aided by analogy, could never lead to their discovery. To divine these enigmas we must be guided by theoretical ideas founded on a *true* hypothesis. The theory of luminous vibrations presents this character, and these precious advantages; for to it we owe the discovery of optical laws the most complicated and most difficult to divine.'

Physicists who embraced the barren emission theory had nothing but their own native capacity and quickness of observation to rely upon. Fresnel having once seized the conditions of the true undulatory theory, as previously stated by Young, was enabled by the mere manipulation of his mathematical symbols to foresee many of the complicated phenomena of light. Who could possibly suppose, or even believe on the ground of mere common sense, that by stopping a large portion of the rays passing through a circular aperture, the illumination of a point upon a screen behind the aperture might be many times multiplied. Yet this paradoxical effect was predicted by Fresnel, and verified both by himself, and in a careful repetition of the experiment in later years, by Billet. Comparatively few persons even now are aware that in the very middle point of the shadow of an opaque circular disc is a point of light sensibly as bright as if no disc had been interposed. This startling fact was deduced from Fresnel's theory by Poisson, and was then verified experimentally by Arago. Airy, again, was led by pure theory to predict that Newton's rings would present a modified appearance if produced between a lens of glass and a plate of metal. This effect happened to have been observed fifteen years before by Arago, unknown to Airy; but another prediction of Airy, that there would be a further modification of the rings when made between two substances of very different refractive indices, was verified by subsequent trial with a diamond. A reversal of the rings takes place

when the space intervening between the plates is filled with a substance of intermediate refractive power, another phenomenon predicted by theory and verified by experiment as Sir John Herschel has described. There is hardly a limit to the number of other complicated effects of the interference of rays of light under different circumstances which might be deduced from the mathematical expressions, if it were worth while, or which, being previously observed can be explained, as in an interesting case observed by Sir John Herschel and explained by Airy[p].

By a somewhat different effort of scientific foresight, Fresnel discovered that any solid transparent medium might be endowed with the power of double refraction by mere compression. For as he attributed the peculiar refracting power of crystals to the unequal elasticity in different directions, he inferred that unequal elasticity, if artificially produced, would give similar phenomena. With a powerful screw and a piece of glass, he then produced not only the colours due to double refraction, but the actual duplication of images. Thus, by a great scientific generalisation, are the apparently unique properties of Iceland spar shown to belong to all transparent substances under certain conditions[q].

All other predictions in optical science are, however, thrown into the shade by the theoretical discovery of conical refraction by the late Sir W. R. Hamilton, of Dublin. In investigating the passage of light through certain crystals, Hamilton found that Fresnel had slightly misinterpreted his own formulæ, and that, when rightly understood, they indicated a phenomenon of a kind never witnessed. A small ray of light sent into a crystal of arragonite in a particular direction, becomes spread out

[p] Airy's, 'Mathematical Tracts,' 3rd edit. p. 312.

[q] Young's 'Works,' vol. i. p. 412.

into an infinite number of rays, which form a hollow cone within the crystal, and a hollow cylinder when emerging from the opposite side. In another case, a somewhat different, but equally strange, effect is produced. These phenomena are peculiarly interesting, because cones and cylinders of light are not produced in any other cases. They are, in fact, wholly opposed to all analogy, and constitute singular, or exceptional cases, of a kind which we shall afterwards have to consider more fully. Their very strangeness rendered them peculiarly fitted to test the truth of the theory by which they were discovered; and when Professor Lloyd, at Hamilton's request, succeeded, after considerable difficulty, in witnessing the new appearances, no further doubt could remain of the validity of the great theory of waves, which we owe to Huyghens, Young, and Fresnel[r].

Predictions from the Theory of Undulations.

It is curious to reflect that the undulations of light, although so inconceivably rapid and small, admit of more accurate observation and measurement than the waves of any other medium. But so far as we can carry out exact experiments on other kinds of waves, we find the phenomena of interference repeated, and analogy gives considerable powers of prediction. Sir John Herschel was perhaps the first to suggest that two sounds might be made to destroy each other by interference[s]. For if one-half of a wave travelling through a tube could be separated, and conducted by a somewhat longer passage, so as, on rejoining the other half, to be one-quarter of a vibra-

[r] Lloyd's 'Wave Theory,' Part ii. pp. 52–58. Babbage, 'Ninth Bridgwater Treatise,' p. 104, quoting Lloyd, 'Trans. of the Royal Irish Academy,' vol. xvii. Clifton, 'Quarterly Journal of Pure and Applied Mathematics,' January, 1860.

[s] 'Encyclopædia Metropolitana,' art. *Sound*, p. 753.

tion behindhand, the two portions would exactly neutralise each other. This experiment has recently been performed with success by Quincke and König[t]. The interference arising between the waves from the two prongs of a tuning-fork was also predicted by theory, and proved to exist by Weber; indeed it may be observed by merely turning round a vibrating fork close to the ear[u].

It is a plain result of the theory of sound that, if we move rapidly towards a sounding body, or if it move rapidly towards us, the pitch of the sound will be a little more acute; and, *vice versâ*, when the relative motion is in the opposite direction, the pitch will be more grave. It arises from the less or greater intervals of time between the successive strokes of waves upon the auditory nerve, according as the ear moves towards or from the source of sound relatively speaking. This effect was predicted by theory, and afterwards verified by the experiments of M. Buys Ballot, on Dutch railways, and of Mr. Scott Russell, in England[x]. Whenever, indeed, one railway train passes another, on the locomotive of which the whistle is being sounded, the drop in the acuteness of the sound may be noticed at the moment of passing. This change gives the sound a peculiar howling character, which many persons must have noticed. I have calculated that, with two trains travelling thirty miles an hour, the effect would amount to rather more than half a tone, and it would often amount to a tone. A corresponding effect is produced in the case of light undulations, when the eye and the luminous body rapidly approach or recede from each other. It is shown by a slight change in the refrangibility of the rays of light, and a consequent change in the place of the lines of the spectrum, which has been made to give most important and unexpected information con-

[t] Tyndall's 'Sound,' p. 261.

[u] Ibid. p. 273.

[x] Ibid. p. 78.

cerning the relative approach or recession of many stars as regards the earth.

Tides are vast waves, and were the earth's surface entirely covered by an ocean of uniform depth, they would admit of very exact theoretical investigation. The wholly irregular form of the several seas introduces unknown quantities and complexities with which theory cannot cope. Nevertheless, Whewell, observing that the tides of the German Ocean consist of interfering waves, which arrive partly round the north of Scotland and partly through the British Channel, was enabled to predict that at a point about midway between Lowestoft and Brill on the coast of Holland, in latitude 52° 27′ N, and longitude 3 h. 14 m. E, no tides would be found to exist. At that point the two waves would be of exactly the same amount, but in opposite phases, so as to neutralise each other. This assertion was verified by a surveying vessel of the British navy [y].

Prediction in other Sciences.

Generations, or even centuries, will probably elapse before mankind are in possession of a mathematical theory of the constitution of matter as complete and satisfactory as the theory of gravitation. Nevertheless, mathematical physicists have in recent years acquired a fair hold of some of the simple relations of the physical forces to matter, and the proof is found in some remarkable anticipations of curious phenomena which had never been observed. Professor James Thomson deduced from Carnot's theory of heat that the application of pressure would lower the melting-point of ice. He even ventured to assign the amount of this effect, and his statement was

y Whewell's 'History of the Inductive Sciences,' vol. ii. p. 471. Herschel's 'Physical Geography,' § 77.

afterwards verified almost exactly by Sir W. Thomson[z]. 'In this very remarkable speculation, an entirely novel physical phenomenon was *predicted*, in anticipation of any direct experiments on the subject; and the actual observation of the phenomenon was pointed out as a highly interesting object for experimental research.' Just as liquids which expand in solidifying will have the temperature of solidification lowered by pressure, so liquids which contract in solidifying will exhibit the reverse effect. They will be assisted in solidifying, as it were, by pressure, so as to become solid at a higher temperature, as the pressure is greater. This latter result was verified by Bunsen and Hopkins, in the case of paraffin, spermaceti, wax, and stearin. The effect upon water has more recently been carried to such an extent by Mousson, that under the vast pressure of 1300 atmospheres, water did not freeze until cooled down to −18° Cent. Another remarkable prediction of Professor Thomson was to the effect that, if a metallic spring be weakened by a rise of temperature, work done against the spring, by bending it, must cause a cooling effect. Although the amount of effect to be expected in a certain apparatus was only about four-thousandths of a degree Centigrade, Dr. Joule[a] succeeded in detecting and measuring the effect to the extent of three-thousandths of a degree, such is the delicacy of modern methods of measurement. I cannot refrain from quoting Dr. Joule's reflections upon this fact[b]. 'Thus even in the above delicate case,' he says, 'is the formula of Professor Thompson completely verified. The mathematical investigation of the thermo-elastic qualities of metals has enabled my illustrious friend to

[z] Maxwell's 'Theory of Heat,' p. 174. 'Philosophical Magazine,' August, 1850. Third Series, vol. xxxvii. p. 123.

[a] 'Philosophical Transactions,' 1858, vol. cxlviii. p. 127.

[b] Ibid. p. 130.

predict with certainty a whole class of highly interesting phenomena. To him especially do we owe the important advance which has been recently made to a new era in the history of science, when the famous philosophical system of Bacon will be to a great extent superseded, and when, instead of arriving at discovery by induction from experiment, we shall obtain our largest accessions of new facts by reasoning deductively from fundamental principles.'

The theory of electricity is a necessary part of the general theory of matter, and is rapidly acquiring the power of prevision. As soon as Wheatstone had proved experimentally that the conduction of electricity occupied time, Faraday remarked in 1838, with wonderful sagacity, that if the conducting wires were connected with the coatings of a large Leyden jar, the rapidity of conduction would be lessened. This prediction remained unverified for sixteen years, until the submarine cable was laid beneath the Channel. A considerable retardation of the electric spark was then detected by Siemens and Latimer Clark, and Faraday at once pointed out that the wire surrounded by water resembles a Leyden jar on a large scale, so that each message sent through the cable verified his remark of 1838[c].

The joint relations of heat and electricity to the metals constitute almost a new science of thermo-electricity. Sir W. Thompson was enabled by theory to anticipate the following curious effect, namely, that an electric current passing in an iron bar from a hot to a cold part produces a cooling effect, but in a copper bar the effect is exactly opposite in character, that is the bar becomes heated[d]. The action of crystals with regard to heat and electricity was partly foreseen on the grounds of theory by Poisson.

c Tyndall's 'Faraday,' pp. 73, 74; 'Life of Faraday,' vol. ii. pp. 82, 83.

d Tait's 'Thermodynamics,' p. 77.

Chemistry, although to a great extent an empirical science, has not been without prophetical triumphs. The existence of the metals potassium and sodium was foreseen by Lavoisier, and their elimination by Davy was one of the chief *experimenta crucis* which established Lavoisier's system. The existence of many other metals which eye had never seen was almost a necessary inference, and theory has not been found at fault. No sooner, too, had a theory of organic compounds been conceived by Professor A. W. Williamson than he foretold the formation of a complex substance consisting of water in which both atoms of hydrogen are replaced by atoms of acetyle. This substance, known as the acetic anhydride, was afterwards produced by Gerhardt. In the subsequent progress of organic chemistry occurrences of this kind have been multiplied almost indefinitely. The theoretical chemist by the classification of his specimens and the manipulation of his formulæ can plan out as it were the creation of whole series of unknown oils, acids, alcohols, and such like products, just as a designer might draw out a multitude of patterns. The formation of many such substances is a matter of course, but there is an interesting prediction given by Hofmann, concerning the possible existence of new compounds of sulphur and selenium, and even oxides of ammonium, which it remains for the future to verify[e].

Prediction by Inversion of Cause and Effect.

There is one process of experiment which has so often led to important discoveries as to deserve separate description and illustration—I mean the inversion of Cause and Effect. Thus if A and B in one experiment produce C as a consequent, then antecedents of the nature of B

[e] Hofmann's 'Introduction to Chemistry,' pp. 224, 225.

and C may usually be made to produce a consequent of the nature of A inverted in direction. When we apply heat to a gas it tends to expand; hence if we allow the gas to expand by its own elastic force, cold is the result; that is B (air) and C (expansion) produce the negative of A (heat). Or again, B (air) and compression, the negative of C, produce A (heat). Similar results may be expected in a multitude of cases. It is a most familiar law that heat expands iron and nearly all solid bodies. What may be expected, then, if instead of increasing the length of an iron bar by heat we use mechanical force and stretch the bar? Having the bar and the former consequent, expansion, we should expect the negative of the former antecedent, namely cold. The truth of this inference was proved by Dr. Joule, who investigated the amount of the effect with his usual skill[f].

This inversion of cause and effect in the case of heat may be itself again inverted in a highly curious manner. It happens that there are a few substances which are unexplained exceptions to the general law of expansion by heat. India-rubber especially is remarkable for *contracting* when heated. Since, then, iron and india-rubber are oppositely related to heat, we may expect that as distension of the iron produced cold, distension of the india-rubber will produce heat. This is actually found to be the case, and any one may detect the effect by suddenly stretching an india-rubber band while the middle part is in the mouth. Whenever stretched it will be found to grow slightly warm, and when relaxed cold.

The reader will readily see that many of the scientific predictions mentioned in preceding sections were due to the principle of inversion; for instance, Professor Thompson's speculations on the relation of pressure and the melting-point. But many other illustrations could be

[f] 'Philosophical Transactions,' (1855) vol. cxlv. pp. 100, &c.

adduced. The usual agent by which we melt or liquefy a substance is heat; but if we can melt a substance without heat, then we may expect the negative of heat as an effect. This is the foundation of all freezing mixtures. The affinity of salt for water causes it to melt snow or ice, and may thus reduce the temperature to Fahrenheit's zero. Calcium chloride has so much higher an attraction for water that a temperature of $-50°$ Fahr. may thus be attained. Even the solution of a certain alloy of lead, tin, and bismuth in mercury, may be made to reduce the temperature from 63° to 14° Fahr. All the other modes of producing cold are inversions of more familiar uses of heat. Carré's freezing machine is an inverted distilling apparatus, the distillation being occasioned by chemical affinity instead of heat. Another kind of freezing machine is the exact inverse of the steam engine.

A very paradoxical effect is due to another inversion. It is hard to believe at the first moment that a current of steam at 212° could raise a body of liquid to a higher temperature than the steam itself possesses. But Mr. Spence has pointed out that if the boiling-point of a saline solution be above 212°, it will continue, on account of its affinity for water, to condense steam when above 212° in temperature. It will condense the steam until heated to the point at which the tension of its vapour is equal to that of the atmosphere, that is, its own boiling-point[8].

Since heat, again, melts ice, we might expect to produce heat by the inverse change from water into ice. Now this is accomplished in the phenomenon of suspended freezing. Water may be cooled in a clean glass vessel many degrees below the freezing-point, and yet retained in the liquid condition. But if disturbed, and especially if brought into contact with a small particle of ice, it immediately

[8] 'Proceedings of the Manchester Philosophical Society,' Feb. 1870.

solidifies and rises in temperature to 32° Fahr. A like effect is still more beautifully displayed in the well known lecture-room experiment, of the suspended crystallization of a solution of sodium sulphate, in which a sudden rise of temperature of 30° or even 40° Fahr. is often manifested.

The science of electricity is full of the most varied and interesting cases of inversion. As Professor Tyndall has remarked, Faraday had a profound belief in the reciprocal relations of the physical forces. The great starting-point of his researches, the discovery of electro-magnetism, was clearly an inversion.

Oersted and Ampère had proved that with an electric current and a magnet in a particular position as antecedents, motion is the consequent. If then a magnet, a wire and *motion* be the antecedents, an *opposite* electric current will be the consequent. It would be an endless task to trace out the results of this fertile relationship when once fully understood. No small part of Faraday's researches was occupied in ascertaining the direct and inverse relations of magnetic and diamagnetic, amorphous and crystalline substances in various circumstances. In all other relations of electricity the principle of inversion holds. The voltameter or the electro-plating cell is the inverse of the galvanic battery. As heat applied to a junction of antimony and bismuth bars produces electricity, it necessarily follows that an electric current passed through such a junction will produce cold. Thus it is apparent that inversion of cause and effect is a most fertile ground of prediction and discovery.

The reader should carefully notice, however, that the inversion of natural phenomena is exactly true only of the character of the effect, not the amount. There is always a waste of energy in every work, because a certain part of it is dissipated in the form of conducted or radiated heat, and escapes beyond our use. Theoretically speaking,

we might imagine a train of magnetic engines and electro-magnetic machines, which should alternately convert the same energy into motion and electricity. Similarly, by a proper arrangement of bars of antimony and bismuth, the same current of electricity might be converted into heat and reconverted into electricity an indefinite number of times. But, practically speaking, there would be an enormous loss of energy at each conversion, so that the ultimate effect would dwindle down to an inconsiderable fraction of the original amount of energy.

Facts known only by Theory.

Of the four classes of facts enumerated in p. 157 the last remains unconsidered. It includes the unverified predictions of science. Scientific prophecy arrests the attention of the world when it refers to such striking events as an eclipse, the appearance of a great comet, or any other phenomenon which every one can verify with his own eyes. But it is surely a greater matter for wonder that in many cases a physicist may describe and measure phenomena which eye cannot see, nor sense of any kind appreciate. In most cases this arises from the effect being too small in amount to affect our organs of sense, or come within the powers of our instruments as at present constructed. There is another class of yet more remarkable cases, where a phenomenon cannot possibly be observed, and yet we can say what it would be if it were observed.

In astronomy, systematic aberration is an effect of the sun's proper motion almost certainly known to exist, but which we have no hope of detecting by observation in the present age of the world. As the earth's motion round the sun combined with the motion of light causes the stars to deviate apparently from their true positions to the extent of about 18″ at the most, so the motion of the

whole planetary system through space must occasion a similar displacement of at most 5″. The ordinary aberration can be readily detected with modern astronomical instruments, because it goes through a yearly change in direction or amount, but the systematic aberration is constant and permanent so long as the planetary system moves uniformly in a sensibly straight line. Only then in the course of ages, when the curvature of the sun's path becomes apparent, can we hope to verify the existence of this kind of aberration. A curious effect also must be produced by the sun's proper motion upon the apparent periods of revolution of the binary stars.

To my mind, some of the most interesting truths in the whole range of science are those which have not been, and in many cases probably can never be, verified by trial. Thus the chemist assigns, with a very high degree of probability the vapour densities of such elements as carbon and silicon, which have never been observed separately in a state of vapour. The chemist also is familiar with the vapour densities of elements at temperatures at which the elements in question never have been, and probably never can be, submitted to experiment in the form of vapour.

Joule and others have calculated the actual velocity of the molecules of a gas, and even the number of collisions which must take place per second during their constant circulation. Sir W. Thomson has not yet given us the exact absolute magnitudes of the particles of matter, but he has ascertained by several distinct methods the limits within which their magnitudes must lie. Many of such scientific results must for ever be beyond the power of verification by the senses. I have elsewhere had occasion to remark that waves of light, the intimate processes of electrical changes, the properties of the ether which is the base of all phenomena, are necessarily determined

in a hypothetical, but not therefore a less certain manner.

Though only two of the metals, gold and silver, have ever been observed to be transparent, we know on the grounds of theory that they are all more or less so; we can even estimate by theory their refractive indices, and prove that they are exceedingly high. The phenomena of elliptic polarization, and perhaps also the theory of internal radiation[h], depend upon the refractive index, and thus, even when we cannot observe any refracted rays, we can indirectly learn how they would be refracted.

In many cases large quantities of electricity must be produced, which we cannot observe because it is instantly discharged. In the common electric machine the cylinder and rubber are made of non-conductors, so that we can separate and accumulate the electricity. But even a little damp, by serving as a conductor, prevents this separation from enduring any sensible time. Hence there is little or no doubt that when we rub two good conductors against each other, for instance two pieces of metals, much electricity is produced, but instantaneously converted into some other form of energy. Dr. Joule, indeed, believes that all the heat of friction is but transmuted electricity.

As regards phenomena of insensible amount, Nature is absolutely full of them. We must, in fact, regard those considerable changes which we can observe as the comparatively speaking infinitely rare aggregates of minuter changes. On a little reflection we must allow that no object known to us remains for two instants of exactly the same temperature. If so, the dimensions of objects must be in a perpetual state of variation. The minor planetary and lunar perturbations are indefinitely, or rather infinitely numerous, but usually too small to be detected by

[h] Balfour Stewart, 'Elementary Treatise on Heat,' 1st edit. p. 198.

observation, although their amounts may be confidently assigned by theory. There is every reason to believe that chemical and electric actions of almost indefinitely small amount, are constantly in progress. The hardest and most fixed substances, if reduced to sufficiently small particles, and diffused in pure water, manifest oscillatory movements which must be due to chemical and electric changes, so slight that they may go on for years without affecting appreciably the weight of the particles. The earth's magnetism must affect more or less every object which we handle. As Professor Tyndall remarks, 'An upright iron stone influenced by the earth's magnetism becomes a magnet, with its bottom a north and its top a south pole. Doubtless, though in an immensely feebler degree, every erect marble statue is a true diamagnet, with its head a north pole and its feet a south pole. The same is certainly true of man as he stands upon the earth's surface, for all the tissues of the human body are diamagnetic[i].' The sun's light produces a very quick and perceptible effect upon the photographic plate; in all probability it has a much less effect upon a great variety of substances. We may regard every apparent phenomenon as but an exaggerated and conspicuous case of a process which is, in indefinitely more numerous cases, beyond the means of observation. Yet in a great proportion of these cases exact calculation will enable us to estimate the amount of the phenomena, if it is of sufficient interest for us to do so.

[i] 'Philosophical Transactions,' vol. cxlvi. p. 249.

CHAPTER XXV.

ACCORDANCE OF QUANTITATIVE THEORIES AND EXPERIMENTS.

In the preceding chapter we found that facts may be classed under four heads as regards their connexion with theory, and our powers of explanation or prediction. The facts hitherto considered were generally of a qualitative rather than a quantitative nature; but when we look exclusively to the quantity of a phenomenon, and the various modes in which we may estimate or establish its amount, almost the same system of classification will hold good. There will, however, be five possible cases:—

(1) We may directly and empirically measure a phenomenon, without being able to explain why it should have any particular quantity, or to connect it by theory with other quantities.

(2) In a considerable number of cases we can theoretically predict the existence of a phenomenon, but may be unable to assign its amount, except by direct measurement, or to explain the amount theoretically when thus ascertained.

(3) We may measure a quantity, and afterwards explain it as related to other quantities, or as governed by known quantitative laws.

(4) We may predict the quantity of an effect on theoretical grounds, and afterwards confirm the prediction by direct measurement.

(5) We may indirectly predict or determine the quantity of an effect without being able to verify it by experiment.

These various classes of quantitative facts might be illustrated by an almost infinite number of interesting points in the history of physical science. Philosophical prophecies especially serve to show the mastery which is sometimes attained over the secrets of nature, and to convince the least intelligent of the value of theory.

Empirical Measurements.

Under the first head of purely empirical measurements, which have not been brought under any theoretical system, may be placed the great bulk of quantitative facts recorded by scientific observers. The tables of numerical results which abound in books on chemistry and physics, the huge quartos containing the observations of public observatories, the multitudinous tables of meteorological observations, which are continually being compiled and printed, the more abstruse results concerning terrestrial magnetism—such results of measurement, for the most part, remain empirical, either because theory is defective, or the labour of calculation and comparison is too formidable. In the Greenwich Observatory, indeed, the salutary practice has been maintained by the present Astronomer Royal, of always reducing the observations, and comparing them with the recognised theories of motion of the several bodies. The divergences from theory thus afford a constant supply of material for the discovery of errors or of new phenomena; in short, the observations have been turned to the use for which they were intended. But it is to be feared that other establishments are too often engaged in merely recording numbers of which no real use is made, because the labour of reduc-

tion and comparison with theory, in detail, is far too great for private inquirers to undertake. In meteorology, especially, an enormous waste of labour and money is taking place, only a very small fraction of the results recorded being ever used for the advancement of the science. For one meteorologist like Quetelet, Dove, or Baxendell, who devotes himself to the truly useful labour of reducing other people's observations, there are hundreds who are under the delusion that they are advancing science by merely loading our book-shelves with numerical fables.

Purely empirical measurements may often indeed have a direct practical value, as when tables of the specific gravity, or strength of materials, assist the engineer; the specific gravities of mixtures of water with acids, alcohols, salts, &c., are useful in chemical manufactories, custom-house guaging, &c.; observations of rain-fall are requisite for questions of water supply; the refractive index of various kinds of glass must be known in making achromatic lenses; but in all such cases the use made of the measurements is not scientific, but practical. It may probably be asserted with truth, that no number which remains entirely isolated, and uncompared by theory with other numbers, is of any really scientific value. Having tried the tensile strength of a piece of iron in a particular condition, we know what will be the strength of the same kind of iron in a similar condition, provided we can ever meet with that exact kind of iron again; but we cannot argue from piece to piece, or lay down any laws exactly connecting the strength of iron with the quantity of its impurities.

It is to be feared that almost the whole bulk of statistical numbers, whether commercial, vital, or moral, is at present, and probably will long continue, of little scientific value.

Quantities indicated by Theory, but Empirically Measured.

In many cases we are able to foresee the existence of a quantitative effect, on the ground of general principles, but are unable, either from the want of numerical data, or from the entire absence of any mathematical theory, to assign the amount of such effect. We then have recourse to direct experiment to determine its amount. Whether we argued from the oceanic tides by analogy, or more generally from the theory of gravitation, there could be no doubt that atmospheric tides of some amount, depending on the apparent heights of the sun and the moon, must occur in the atmosphere. Theory, however, even in the hands of Laplace, was not able to overcome the complicated mechanical conditions of the atmosphere, and predict the amount of such tides; and, on the other hand, these amounts were so small, and were so masked by far larger undulations arising from the heating power of the sun, and from other meteorological disturbances, that they would probably have never been discovered by purely empirical observations. Theory having, however, indicated their existence, it was easy to make series of barometrical observations in places selected so as to be as free as possible from casual fluctuations, and then by the suitable application of the method of means to detect the small effects in question. The principal lunar atmospheric tide was thus proved to amount to between ·003 and ·004 inch[a].

Theory, in fact, yields the greatest possible assistance in applying the method of means. For if we have a great number of empirical measurements, each representing the joint effect of a number of causes, our object will be to

[a] Grant's 'History of Physical Astronomy,' p. 162.

take the mean of all those in which the effect to be measured is present, and compare it with the mean of the remainder in which the effect is absent, or acts, it may be, in the opposite direction. The difference will then represent the amount of the effect, or double the amount respectively. Thus, in the case of the atmospheric tides, we take the mean of all the observations when the moon was on the meridian, and compare it with the mean of all observations when she was on the horizon. In this case we trust to chance that all other effects will lie about as often in one direction as the other in the drawing of each mean, and will neutralise themselves. It will be a great advantage, however, to be able to decide by theory when each principal disturbing effect is present or absent; for the means may then be so drawn as surely to separate each such effect, leaving only very minor and casual divergences to the law of error. Thus, if there be three principal effects, and we draw means giving respectively the sum of all three, the sum of the first two, and the sum of the last two, then we gain three simple equations, by the solution of which each quantity is determined.

Explained Results of Measurement.

The second class of measured phenomena contains those which, after being determined in a direct and purely empirical application of measuring instruments, are afterwards shown to agree with some hypothetical explanation. Such results are turned to their proper use, and several different advantages may arise from the comparison. The correspondence with theory will seldom or never be absolutely precise; and, even if it be so, the coincidence must be regarded as accidental. If the divergences between theory and experiment be comparatively small, and variable in amount and direction, they may often be safely

attributed to various inconsiderable sources of error in the experimental processes. The strict method of procedure is to calculate, if possible, the probable error of the mean of the observed results (vol. i. p. 451), and then observe whether the theoretical result falls within the limits of probable error. If it does, and if, as we may say, the experimental results agree as well with theory as they agree with each other, then the probability of the theory is much increased, and we may employ the theory with more confidence in the anticipation of further results. The probable error, it should be remembered, gives a measure only of the effects of incidental and variable sources of error, but in no way or degree indicates the amount of fixed causes of error. Thus, if the mean results of any two modes of determining a quantity are so far apart that the limits of probable error do not overlap, we may infer the probable existence of some overlooked source of permanent error in one or both modes. We will further consider in a subsequent section the accordance or discordance of measurements.

Quantities determined by Theory and verified by Measurement.

One of the most satisfactory tests of a theory consists in its application not only to predict the nature of a phenomenon, and the circumstances in which it may be observed, but also to assign the precise quantity of the phenomenon. If we can subsequently apply accurate instruments and measure the amount of the phenomenon witnessed, we have an excellent opportunity of verifying or negativing the theory. It was in this manner that Newton first attempted to verify his theory of gravitation. He knew approximately the velocity produced in falling bodies at the earth's surface, and if the law of the inverse

square of the distance held true, and the reputed distance of the moon was correct, he could infer that the moon ought to fall towards the earth at the rate of fifteen feet in one minute. Now, the actual divergence of the moon from the tangent of its orbit appeared to amount only to thirteen feet in one minute, and there was a discrepancy of two feet in fifteen, which caused Newton to lay 'aside at that time any further thoughts of this matter.' Many years afterwards, probably fifteen or sixteen years, Newton obtained more precise data from which he could calculate the size of the moon's orbit, and he then found the discrepancy to be inconsiderable.

His theory of gravitation was then verified so far as the moon was concerned; but this was to him only the beginning of a long course of deductive calculations, each ending in a verification. If the earth and moon attract each other, and also the sun and the earth, similarly there is no reason why the sun and moon should not attract each other. Newton followed out the consequences of this inference, and showed that the moon would not move as if attracted by the earth only, but sometimes faster and sometimes slower. Comparisons with Flamsteed's observations of the moon showed that such was the case. Newton argued again, that as the waters of the ocean are not rigidly attached to the earth, they might attract the moon, and be attracted in return, independently of the rest of the earth. Certain daily motions would then be caused thereby exactly resembling the tides, and there were the tides to verify the fact. It was the almost superhuman power with which he traced out geometrically the consequences of his theory, and submitted them to repeated comparison with experience, which constitutes his pre-eminence over all philosophers [b].

The whole progress of physical astronomy has consisted

[b] 'Elementary Lessons in Logic,' p. 262.

in a succession of predictions grounded on the theory of gravitation as to the inequalities of the planetary movements caused by mutual perturbations. These inequalities are so numerous, so small, and so complicated in character, that it would be an almost hopeless task to attempt to discover them empirically or tentatively by the comparison and classification of observations. But theory pretty easily indicates the period and general nature of the inequality to be detected, and by elaborate calculations even the amount of the effect may be assigned. Thus the inequality arising from the attraction of Venus and the earth was estimated by Sir George Airy to amount to no more than a few seconds at its maximum, while the period is no less than 240 years. Nevertheless, the indirect effects of this inequality upon the moon's motion are considerable, and are entirely verified in the lunar theory. Although prediction by theory is the general rule in physical astronomy, yet the empirical investigation of divergences from theory sometimes discloses effects which had been overlooked, or points out residual effects of unknown origin.

Quantities determined by Theory and not verified.

It will continually happen that we are able, from certain measured phenomena and a correct theory, to determine the amount of some other phenomenon which we may either be unable to measure at all, or to measure with an accuracy corresponding to that required to verify the prediction. Thus Laplace having worked out an almost complete theory of the motions of Jupiter's satellites on the hypothesis of gravitation, found that these motions were greatly affected by the spheroidal form of Jupiter. Hence from the motions of the satellites, which can be observed with great accuracy owing to the frequent

eclipses and transits, he was able to argue inversely, and assign the ellipticity of the planet's section by theory. The ratio of the polar and equatorial axes thus determined was very nearly that of 13 to 14; and it agrees well with such direct micrometrical measurements of the planet as have been made; but Laplace believed that the theory gave a more accurate result than direct observation could yield, so that the theory could hardly be said to admit of direct verification.

The specific heat of air was believed on the grounds of direct experiment to amount to 0·2669, the specific heat of water being taken as unity; but the methods of experiment were open to considerable causes of error. The late Professor Rankine showed in 1850 that it was possible to calculate from the mechanical equivalent of heat, and from other thermodynamic data, what this number should be, and he found for it 0·2378. This determination was at the time accepted by him and others as the most satisfactory result, although not verified; subsequently in 1853 Regnault obtained by direct experiment the number 0·2377, proving that the prediction had been well grounded.

It will be readily seen that in purely quantitative questions verification will be a matter of degree and probability. A less accurate method of measurement cannot verify the results of a more accurate method, so that if we arrive at a determination of the same physical quantity in several distinct modes it will often become a delicate matter of investigation to decide which result is most reliable, and should be used for the indirect determination of other quantities. For instance, Joule's and Thomson's ingenious experiments upon the thermal phenomena of fluids in motion[c] involved, as one physical constant, the mechanical equivalent of heat; if requisite,

[c] 'Philosophical Transactions' (1854), vol. cxliv. p. 364.

then, they might have been used to predict or to correct that most important constant. But if other more direct methods of experiment give the mechanical equivalent of heat with superior accuracy, then the experiments on fluids will be turned to a better use in detecting and assigning various quantities relating to the theory of fluids. We will further consider questions of this kind in succeeding sections.

There are of course many quantities assigned on theoretical grounds which we are quite unable to verify with corresponding accuracy. The thickness of a film of gold leaf, the average depths of the oceans, the velocity of a star's approach to or regression from the earth as inferred from spectroscopic data, or other quantities indirectly determined (see vol. i. pp. 345–349), might be cases in point; but many others might be quoted where direct verification seems impossible. Newton and many subsequent physicists have accurately measured the lengths of light undulations, and by several distinct methods we learn the velocity with which light travels. Since an undulation of the middle green is about five ten-millionths of a metre in length, and travels at the rate of nearly 300,000,000 of metres per second, it necessarily follows that about 600,000,000,000,000 undulations must strike in one second the retina of an eye which perceives such light. But how are we to verify such an astounding calculation by directly counting pulses which recur six hundred billions of times in a second?

Discordance of Theory and Experiment.

When a distinct want of accordance is found to exist between the results of theory and direct measurement, several interesting questions may arise as to the mode in which we can account for this discordance. The ultimate

explanation of the discrepancy may be accomplished in any one of at least four distinct ways, as follows :—

(1) The direct measurement may be erroneous owing to various sources of casual error.

(2) The theory may be correct so far as regards the general form of the supposed laws, but some of the constant numbers or other quantitative data employed in the theoretical calculations may be inaccurate.

(3) The theory may be false, in the sense that the forms of the mathematical equations assumed to express the laws of nature are incorrect.

(4) The theory and the involved quantities may be approximately accurate, but some regular unknown cause may have interfered, so that the divergence may be regarded as a *residual effect* representing possibly a new and interesting phenomenon.

No precise rules can be laid down as to the best mode of proceeding to explain the divergence, and the experimentalist will have to depend upon his own insight and knowledge; but the following general recommendations may perhaps be made.

In the first place, if the experimental measurements are not numerous, repeat them and take a more extensive mean result, the probable accuracy of which, as regards freedom from casual errors of experiment, will increase as the square root of the number of experiments. Supposing that no considerable modification of the result is thus effected, we may suspect the existence of some more deep-seated and constant source of error in our method of measurement. The next resource will be to change the size and form of the apparatus employed, and to introduce various modifications in the materials employed or in the course of procedure, in the hope, as before explained (vol. i. p. 462), that some cause of constant error may thus be removed. If the inconsistency with theory still re-

mains unreduced we may attempt to invent some widely different mode of arriving at the same physical quantity, so that we may be almost sure that the same cause of error will not affect both the new and old results. In some cases it is possible to find five or six essentially different modes of arriving at the same determination.

Supposing that the discrepancy still exists we may well begin to suspect that our direct measurements are correct, but that the data employed in the theoretical calculations are inaccurate. We must now review the grounds on which these data depend, consisting as they must ultimately do of direct measurements. A comparison of the various recorded results will show the degree of probability attaching to the mean result employed; and if there is any ground for imagining the existence of error, we should repeat the observations, and vary the forms of experiment just as in the case of the previous direct measurements. The continued existence of the discrepancy must show that we have not really attained to a complete acquaintance with the theory of the causes in action, but two different cases still remain. We may have misunderstood the action of those causes which do exist, or we may have overlooked the existence of one or more other causes. In the first case our hypothesis appears to be wrongly chosen and inapplicable; but whether we are to reject it will depend upon whether we can form any other hypothesis which yields a more accurate accordance. The probability of an hypothesis, it will be remembered (vol. i. p. 279), is to be judged entirely by the probability that if the supposed causes exist the observed result follows; but as there is now very little probability of reconciling the original hypothesis with our direct measurements the field is open for new hypotheses, and any one which gives a closer accordance with measurement will so far have claims to attention. Of course we must never estimate

the probability of an hypothesis merely by its accordance with a few results only. Its general analogy and accordance with other known laws of nature, and the fact that it does not conflict with any other probable theories, must be taken into account, as we shall see in the next book. The requisite condition of a good hypothesis, that it must admit of the deduction of facts verified in observation, must be interpreted in the widest possible manner, as including all ways in which there may be accordance or discordance.

All our attempts at reconciliation having failed, the only conclusion we can come to is that some unknown cause of a new character exists. If the measurements be accurate and the theory probable, then there remains a *residual phenomenon*, which, being devoid of theoretical explanation, must be set down as a new empirical fact worthy of deliberate investigation. As a matter of fact these outstanding residual discrepancies have often been found to involve new discoveries of the greatest importance.

Accordance of Measurements of Astronomical Distances.

One of the most instructive instances which we could meet, as regards the manner in which different measurements confirm or check each other, is furnished by the determination of the velocity of light, and the dimensions of the planetary system. Roemer first discovered that light requires time in travelling, by observing that the eclipses of Jupiter's satellites, although they of course occur at fixed moments of absolute time, are visible at different moments in different parts of the earth's orbit, according to the distance of the earth and Jupiter. The time occupied by light in traversing the mean semi-diameter of the earth's orbit is found to be about eight minutes. The mean distance of the sun and earth was

long assumed by astronomers as being about 95,274,000 miles, this result being deduced by Bessel from the observations of the transit of Venus, which occurred in 1769, and which were found to give the solar parallax, or what is the same thing, the apparent size of the earth as seen from the sun, as equal to $8''{\cdot}578$. Now, dividing the mean distance of the sun and earth by the number of seconds in $8^m.\ 13^s.3$ we find the velocity of light to be about 192,000 miles per second.

Nearly the same result was obtained in an apparently very different manner. The aberration of light is the apparent change in the direction of a ray of light owing to the composition of its motion with that of the earth's motion round the sun. If we know the amount of aberration and the mean velocity of the earth we can very simply estimate that of light which is thus found to be 191,102 miles (166,072 geographical miles) per second. Now this determination depends upon an entirely new physical quantity, that of aberration, which is ascertained by direct observation of the stars, so that the close accordance of the estimates of the velocity of light as thus arrived at by different methods might seem to leave little room for doubt, the difference being less than one per cent.

Nevertheless, experimentalists were not satisfied until they had succeeded in actually measuring the velocity of light by direct experiments performed upon the earth's surface. Fizeau, by a rapidly revolving toothed wheel, estimated the velocity at 195,920 miles per second. As this result differed by about one part in sixty from estimates previously accepted, there was thought to be room for further investigation. The revolving mirror, previously used by Mr. Wheatstone in measuring the velocity of electricity, was now applied in a more refined manner by Fizeau and by Foucault to determine the velocity of light. The latter physicist finally came to the startling

conclusion that the velocity was not really more than 185,172 miles per second. No repetition of the experiment as thus performed would shake this result, and there was accordingly a discrepancy between the two astronomical and the experimental results of about 7000 miles per second demanding explanation.

Now a very little consideration shows that both the astronomical determinations involve the magnitude of the earth's orbit as one datum, because our estimate of the earth's velocity in its orbit depends upon our estimate of the sun's mean distance. Accordingly as regards this quantity the two astronomical results must count only for one. Though the transit of Venus had been considered to give the best data for the calculation of the sun's parallax and distance, yet astronomers had not neglected other less favourable opportunities. Thus Hansen, calculating from certain inequalities in the moon's motion, had estimated it at 8″·916; Winneke, from observations of Mars, at 8″·964; Leverrier, from the motions of Mars, Venus, and the moon, at 8″·950. Now these independent results agree much better with each other than with that of Bessel (8″·578) previously received, or that of Encke (8″·58) deduced from the transits of Venus in 1761 and 1769, and though each separately might be worthy of less credit, yet their close accordance renders their mean result (8″·943) probably comparable in probability with that of Bessel. It was further found that if Foucault's value for the velocity of light were assumed to be correct, and the sun's distance were inversely calculated from that and the other requisite data, the sun's parallax would appear to be 8″·960, which closely agreed with the above mean result. This further correspondence of independent results threw the balance of probability strongly against the results of the transit of Venus, and rendered it desirable to reconsider the observations made on that occasion.

Mr. E. J. Stone having re-discussed those observations[d] found that grave oversights had been made in the calculations, which being corrected would alter the estimate of parallax to 8″·91, a quantity in such comparatively close accordance with the other results that astronomers did not hesitate at once to reduce their estimate of the sun's mean distance from 95,274,000 to 91,771,000 miles, although this alteration involved a corresponding correction in the assumed magnitudes and distances of most of the heavenly bodies. The final decision of this question of the ratio between the earth and the visible universe, so far as it can be decided in the present century, must be made at the approaching transits of Venus in 1874 and 1882.

In this important and interesting question the theoretical relations between the velocity of light, the constant of aberration, the sun's parallax, and the sun's mean distance, are of the simplest character, and can hardly be open to any doubt, so that the only doubt was as to which result of observation was the most reliable. Eventually the chief discrepancy was found to arise from misapprehension in the reduction of observations, but we have a satisfactory example of the value of different methods of estimation in leading to the detection of a serious error. Is it not surprising that Foucault by measuring the velocity of light when passing through the space of a few yards, should lead the way to a change in our estimates of the magnitude of the whole universe?

Selection of the best Mode of Measurement.

When we have once obtained a command over a question of physical science by comprehending the theory of the

d 'Monthly Notices of the Royal Astronomical Society,' vol. xxviii. p. 264.

subject, we have often a wide choice opened to us as regards the methods of measurement, which may thenceforth be made to give the most accurate results. If we can only measure one fundamental quantity we may often be able by correct theory to assign with accuracy a great many other quantitative results. Thus, if we can once determine satisfactorily the atomic weights of certain elements, we do not need to determine with equal accuracy the composition and atomic weights of their several compounds. When we have once learnt the relative atomic weights of oxygen and sulphur we can calculate the composition by weight of the several oxides of sulphur. Chemists accordingly select with the greatest care that compound of any two elements which seems to allow of the most accurate analysis so as to give the ratio of their atomic weights. It is obvious that we only need to have the ratio of the atomic weight of each element to that of some other common element, in order to calculate with the greatest ease that of each to each. Moreover the atomic weight stands in simple relation to other quantitative facts. The weights of equal volumes of elementary gases at equal temperature and pressure have the same ratio as the atomic weights; now as nitrogen weighs 14·06 times as much as hydrogen, under such circumstances we may infer that the atomic weight of nitrogen is about 14·06 (probably 14·00) that of hydrogen being unity. There is much evidence, again, to show that the specific heats of elements, and even of compounds, are inversely as their atomic weights, so that these two classes of quantitative data throw light mutually upon each other. In fact the atomic weight, the atomic volume, and the atomic heat of an element, are quantities so closely connected that the determination of any one may lead to that of the others. The chemist accordingly has to solve a most complicated problem in deciding in the case of each of 60 or

70 elements which mode of determination is most accurate. Modern chemistry presents us with an almost infinitely extensive web of numerical ratios developed out of a comparatively few fundamental ratios.

In hygrometry we are presented with a choice among at least four modes of measuring the quantity of aqueous vapour contained in a given bulk of air. We can extract the vapour by absorption in sulphuric acid, and directly weigh its amount; we can place the air in a barometer tube and observe how much the absorption of the vapour alters the elastic force of the air; we can observe the dew point of the air, or the temperature at which the vapour becomes saturated; or, lastly, we can insert a dry and wet bulb thermometer and observe the temperature of an evaporating surface. Now the results of each such mode can be connected by well-established theory with those of the other modes, and we can select for each experiment that mode which is either most accurate or most convenient. The chemical method of direct measurement is probably capable of the greatest accuracy, but is troublesome; the dry and wet bulb thermometer is sufficiently exact for meteorological purposes.

Agreement of Distinct Modes of Measurement.

Many illustrations might be given of the accordance which has been found to exist in some cases between the results of entirely different methods of arriving at the measurement of a physical quantity. While such accordance must, in the absence of any information to the contrary, be regarded as the best possible proof of the approximate correctness of the mean result, yet instances have occurred to show that we can never take too much trouble in confirming experimental results of great importance. Even when three or more distinct methods have given nearly

coincident results, a new method has sometimes disclosed a discrepancy which it is yet impossible to explain.

The ellipticity of the earth is known with very considerable approach to certainty and accuracy, for it has been estimated in three independent ways. The most direct mode is to measure long arcs extending north and south upon the earth's surface, by means of trigonometrical surveys, and then to compare the lengths of these arcs with the amount of their curvature as determined by the observation of the altitude of certain stars at the terminal points. The most probable ellipticity of the earth deduced from all measurements of this kind was estimated by Bessel at $\frac{1}{300}$, though subsequent measurements might lead to a slightly different estimate. The divergence from a globular form causes a small variation in the force of gravity in different parts of the earth's surface, so that exact pendulum observations give the data for an entirely independent estimate of the ellipticity, which is thus found to be $\frac{1}{320}$. In the third place the spheroidal protuberance about the earth's equator leads to a certain inequality in the moon's motion, as shown by Laplace; and from the amount of that inequality, as given by observations, Laplace was enabled to calculate back to the amount of its cause. He thus inferred that the ellipticity is $\frac{1}{305}$, which lies between the two numbers previously given, and was considered by him to be the most satisfactory conclusion. In this case the accordance is both close and undisturbed by any other or subsequent results, so that we are obliged to accept Laplace's result as a highly probable and accurate one.

The mean density of the earth is another constant quantity of the highest importance, because it forms the starting-point for the determination of the masses of all the other

heavenly bodies. Physicists have accordingly bestowed a great amount of labour upon the exact estimation of this density, consisting in the exact comparison of the gravity of the whole globe with the gravity of some selected body of matter, of which the mass, or what comes to the same thing, the density compared with water, is known more or less exactly. But this body of matter may be variously chosen; it may consist of a heavy ball of lead, or a mountain, or a portion of the earth's strata, and the methods of experiment are so very different in these different cases that they may be regarded as giving entirely independent results.

The mutual gravitation of two balls, or other small objects at the earth's surface, is so exceedingly small compared with their gravitation towards the immense mass of the earth, that it is usually quite imperceptible, and although asserted by Newton to exist, on the ground of theory, was never detected until the end of the 18th century. Michell attached two small balls to the extremities of a delicately suspended torsion balance, and then bringing heavy balls of lead alternately to each side of these small balls was able to detect a certain slight deflection of the torsion balance, which was a new verification of the theory of gravitation. Cavendish carried out the experiment with more care, and by estimating the actual gravitation of the balls by treating the torsion balance as a pendulum, and then taking into account the respective distances of the balls from each other and from the centre of the earth, was able to assign 5·48 (or as recomputed by Baily, 5·448) as the probable mean density of the earth. Newton's sagacious guess to the effect that the density of the earth was between five and six times that of water, was thus remarkably confirmed. The same kind of experiment repeated by Reich gave 5·438. Baily having again performed the experiment with every

possible refinement obtained a slightly higher number, 5·660.

A different method of procedure consisted in ascertaining the effect of a mountain mass in deflecting the plumb-line; for assuming that we can determine the dimensions and mean density of the mountain the plumb-line enables us to compare its mass with that of the whole earth. The Mountain Schehallien was selected for such an experiment, and the observations and calculations performed by Maskelyne, Hutton, and Playfair, gave as the most probable result, 4·713. The difference is considerable and the result is valuable, because the instrumental operations are of an entirely different character from those of Cavendish and Baily's experiments. Sir Henry James' similar determination from the attraction of Arthur's Seat gave 5·14.

A third distinct method consists in determining the force of gravity at points elevated above the surface of the earth on mountain ranges, or sunk below it in mines. Carlini experimented with a pendulum at the hospice of Mont Cenis, 6375 feet above the sea, and by comparing the attractive forces of the earth and the mountains, found the density to be still smaller, namely, 4·39, or as corrected by Giulio, 4·950. Lastly, the Astronomer Royal has on two occasions adopted the opposite method of observing a pendulum at the bottom of a deep mine, so as to compare the density of the strata penetrated with the density of the whole earth. On the second occasion he carried his method into effect at the Harton Colliery, 1260 feet deep; all that could be accomplished by skill in measurement and careful consideration of all the causes of error, was accomplished in this elaborate series of observations[e] (see vol. i. p. 340). No doubt Sir George Airy was much surprised and perplexed when he found that his new result

[e] 'Philosophical Transactions' (1856), vol. cxlvi. p. 342.

considerably exceeded that obtained by any other method, being no less than 6·566, or 6·623 as finally corrected.

In 1844 Sir John Herschel remarked in his Memoir of Francis Baily[f], that 'the mean specific gravity of this our planet is, in all human probability, quite as well determined as that of an ordinary hand-specimen in a mineralogical cabinet,—a marvellous result, which should teach us to despair of nothing which lies within the compass of number, weight, and measure.' But at the same time he pointed out that Baily's final result, of which the probable error was only 0·0032, was the highest of all determinations then known, and Airy's investigation has since given a much higher result, quite beyond the limits of probable error of any of the previous experiments. If we treat all determinations yet made as of equal weight, the simple mean is about 5·45, the mean error nearly 0·5, and the probable error almost 0·2, so that it is as likely as not that the truth lies between 5·65 and 5·25 on this view of the matter. But it is remarkable that the two most recent and careful series of observations, by Baily and Airy[g], lie beyond these limits, and as with the increase of care the estimate rises, it seems requisite to reject the earlier results, and look upon the question as still requiring further investigation. In this case we learn an impressive lesson concerning the value of repeated determinations by distinct methods in disabusing our minds of the reliance which we are only too apt to place in results which show a certain degree of coincidence.

Since the establishment of the dynamical theory of heat it has become a matter of the greatest importance to determine with accuracy and high probability the mechanical equivalent of heat, or the quantity of energy which

f 'Monthly Notices of the Royal Astronomical Society' for 8th Nov. 1844, No. X, vol. vi. p. 89.

g 'Philosophical Magazine,' 2nd Series, vol. xxvi. p. 61.

must be given, or received, in a definite change of temperature effected in a definite quantity of a standard substance, such as water. No less than seven almost entirely distinct modes of determining this constant have been tried. Dr. Joule first ascertained by the friction of water that to raise the temperature of one kilogram of water through one degree centigrade, we must employ energy sufficient to raise 424 kilograms through the height of one metre against the force of gravity at the earth's surface. Joule, Mayer, Clausius[h], Favre and other experimentalists have made various other determinations by less direct methods, and their results may be thus summed up[i].

Friction	424 413
Mechanical properties of gases . .	426
Work done by a steam engine . .	413
Heat evolved by induced electric currents	452
Heat evolved by electro-magnetic engine	443
Heat evolved in the circuit of a battery	420
Heat evolved by an electric current .	400

Considering the diverse and in many cases difficult methods of observation, these results exhibit satisfactory accordance, and their mean (423·9) comes very close to the number derived by Dr. Joule from the apparently most accurate method. The constant generally assumed as the most probable result is 423·55 kilogrammetres, or gramme metres, if the quantity of water heated 1° Cent. be one gramme instead of a kilogramme.

[h] Clausius, 'Philosophical Magazine,' 4th Series, vol. ii. p. 119.

[i] Watts' 'Dictionary of Chemistry,' vol. iii. p. 129.

Residual Phenomena.

Even when all the experimental data employed in the verification of a theory are sufficiently accurate, and the theory itself is sound, there may still exist discrepancies demanding further investigation. Sir John Herschel was perhaps the first who pointed out the importance of such outstanding quantities, and called them *residual phenomena*[j]. Now if the observations and the theory be really correct, such discrepancies must be due to the incompleteness of our knowledge of the causes in action, and the ultimate explanation must consist in showing that there is in action

(1) Some agent of known nature whose presence was not suspected.

(2) Some new agent of unknown nature.

In the first case we cannot be said to make any new discovery, for our ultimate success consists merely in reconciling the theory with known facts when our investigation is more comprehensive. But in the second case we meet with a totally new fact, which may lead us to whole realms of new discovery. Take the instance adduced by Sir John Herschel. The theory of Newton and Halley concerning cometary motions was that they were gravitating bodies revolving round the sun in oblique orbits, and the actual return of Halley's Comet, in 1758, sufficiently verified this theory. But, when accurate observations of Encke's Comet came to be made, the verification was not found to be complete. Each time Encke's Comet returned a little sooner than it ought, the period having regularly decreased from 1212·79 days, between 1786 and 1789, to 1210·44 be-

j 'Preliminary Discourse on the study of Natural Philosophy,' §§ 158, 174. 'Outlines of Astronomy,' 4th. edit. § 856.

tween 1855 and 1858. The theory of gravitation alone cannot account for such a continued decrease of period; hence the hypothesis has been started that there is a resisting medium filling the space through which the comet passes. This hypothesis is a *deus ex machinâ* for explaining this solitary phenomenon, and cannot possess any validity or probability unless it can be shown that other phenomena are deducible from it. Many persons have identified this medium with that through which heat undulations pass, but I am not aware that there is anything in the undulatory theory of light to show that the medium would offer resistance to a moving body. If Professor Balfour Stewart can prove that a rotating disc experiences resistance even in a perfectly vacuous receiver, here is an experimental fact which distinctly supports the hypothesis. But in the mean time it is open to question whether other known agents, for instance electricity, may not be brought in, and I have tried to show that if, as seems highly probable, on other grounds, the tail of a comet is an electrical phenomenon, it is almost a necessary result of the theory of the conservation of energy that the comet shall exhibit a loss of energy manifested in a diminution of its mean distance from the sun and its period of revolution[k]. If so, the residual phenomenon seems to confirm an hypothesis as to the nature of the comet itself, rather than that of the medium through which it moves.

In other cases residual phenomena have involved important inferences not recognised at the time. Newton showed how the velocity of sound in the atmosphere could be calculated by a theory of pulses or undulations from the observed tension and density of the air. He inferred that the velocity in the ordinary state of the

[k] 'Proceedings of the Manchester Literary and Philosophical Society,' 28th November 1871, vol. xi. p. 33.

atmosphere at the earth's surface would be 968 feet per second, and very rude experiments made by him in the cloisters of Trinity College seemed to show that this was not far from the truth. Subsequently it was ascertained by other experimentalists that the velocity of sound was more nearly 1142 feet, and the discrepancy being no less than one sixth part of the whole was far too much to attribute to casual errors in the numerical data. Newton attempted to explain away this discrepancy by hypotheses as to the relations of the molecules of air, but without success.

Many new investigations having been made from time to time concerning the velocity of sound, both as observed experimentally and as calculated from theory, it was found that each of Newton's results was inaccurate, the theoretical velocity being 916 feet per second, and the real velocity about 1090 feet. The discrepancy therefore remained as serious as ever, and it was not until the year 1816 that Laplace showed it to be due to the heat developed by the sudden compression of the air in the passage of the wave, this heat having the effect of increasing the elasticity of the air and accelerating the motion of the impulse. It is now perceived that this discrepancy really involved the whole doctrine of the equivalence of heat and energy, and the discrepancy was applied by Mayer, at least by implication, to give an estimate of the mechanical equivalent of heat. The estimate thus derived agrees satisfactorily with independent and more direct determinations by Dr. Joule and other physicists, so that the explanation of the residual discrepancy which so exercised Newton's ingenuity is now complete.

As Sir John Herschel observed, almost all the great astronomical discoveries have been first disclosed in the form of residual differences. It is the practice at well-

conducted observatories to compare the position of the principal heavenly bodies as actually observed with what might have been expected theoretically. This practice was introduced by Halley when Astronomer Royal, and his reduction of the lunar observations gave a series of residual errors from 1722 to 1739, by the examination of which the lunar theory was improved. Most of the greater astronomical variations arising from nutation, aberration, planetary perturbation were in like manner disclosed. The precession of the equinox was perhaps the earliest residual difference observed; the systematic divergence of Uranus from its calculated places was one of the latest, and was the foundation of the remarkable discovery of Neptune by anticipation. We may also class under residual phenomena all the so-called proper motions of the stars. A complete star catalogue, such as that of the British Association, gives a greater or less amount of proper motion for almost every star, consisting in the apparent difference of position of the star as derived from the earliest and latest good observations. But these apparent motions are often due, as is expressly explained by Baily[1], the author of the catalogue, to errors of observation and reduction. In many cases the best astronomical authorities have differed as to the very direction of the supposed proper motion of stars, and as regards the amount of the motion, for instance of α Polaris, the most different estimates have been formed. Residual quantities will of necessity be often so small that their very existence will be doubtful. Only the gradual progress both of theory and of accurate measurement will clearly show whether a discrepancy is to be referred to previous errors of observation and theory or to some new phenomenon. But nothing is more requisite for the progress of science than

[1] 'British Association Catalogue of Stars,' p. 49.

the careful recording and investigation of all such discrepancies. In no part of physical science can we be free from exceptions and outstanding facts, differences and discrepancies of which our present knowledge can give no account. It is among such anomalies that we must look for the key to wholly new realms of facts worthy of discovery. They are like the floating waifs which led Columbus to suspect the existence of the new world.

CHAPTER XXVI.

CHARACTER OF THE EXPERIMENTALIST.

THERE seems to be a tendency to believe that, in the present age, the importance of individual genius is less than it formerly was.

'The individual withers, and the world is more and more.'

Society, it seems to be supposed, has now assumed so highly developed a form, that what was accomplished in past times by the solitary exertions of a single great intellect, may now be gradually worked out by the united labours of an army of investigators. Just as the combination of well-organized power in a modern army entirely supersedes the single-handed bravery of the mediæval knight, so we are to believe that the combination of intellectual labour has superseded the genius of an Archimedes, a Roger Bacon, or a Newton. So-called original research is now regarded almost as a recognised profession, adopted by hundreds of men, and communicated by a regular system of training. All that we need to secure great additions to our knowledge of nature is the erection of great laboratories, museums, and observatories, and the offering of sufficiently great pecuniary rewards to those who can invent new chemical compounds, or detect new species, or discover new comets. Doubtless this is not the real meaning of the eminent men who are now urging upon Government the elaborate endowment of physical

research. They can only mean that the greater the pecuniary and material assistance given to men of science, the greater is the result which the available genius of the country may be expected to produce. Money and opportunities of study can no more produce genius than sunshine and moisture can generate living beings; the inexplicable germ is wanting in both cases. But, just as when the germ is present, the plant will grow more or less vigorously according to the circumstances in which it is placed, so it may be allowed that pecuniary assistance may favour the development of intellect. Public opinion however is not discriminating, and is likely to interpret the agitation for the endowment of science as meaning that science can be evolved from money or labour.

All such notions are, I believe, radically erroneous. In no branch of human affairs, neither in politics, war, literature, industry, nor science, is the influence of genius less considerable than it used to be. It is quite possible that the extension and organization of scientific study, assisted by the printing press and the accelerated means of communication, has increased the rapidity with which new discoveries are made known, and their details worked out by many heads and hands. A Darwin now no sooner propounds original ideas concerning the evolution of animated creatures, than those ideas are discussed and illustrated, and applied by other naturalists in every part of the civilized world. In former days his labours and discoveries would have been hidden for decades of years in scarce manuscripts, and generations would have passed away before his theory had enjoyed the same amount of criticism and corroboration as it has already received in fifteen years. But the general result is that the genius of Darwin is more valuable, not less valuable, than it would formerly have been. The advance of military

science and the organization of enormous and well disciplined armies has not decreased the value of a skilful general; on the contrary, the rank and file are still more in need than they used to be of the guiding power of an ingenious and far-seeing intellect. The swift destruction of the French military power was not due alone to the perfection of the German army, nor to the genius of Moltke; it was due to the combination of a well-disciplined multitude, with a leader of the highest intellectual powers. So in every branch of human affairs the influence of the individual is not withering, but is growing with the extent of the material resources which are at his command.

Nature of Genius.

Turning to our own particular subject, it is a work of undiminished interest to reflect upon those qualities of mind which lead to great advances in natural knowledge. Nothing, indeed, is less amenable than genius to scientific analysis and explanation. Even precise definition is out of the question. Buffon said that 'genius is patience,' and certainly patience is one of its most constant and requisite components. But no one can suppose that patient labour alone will invariably lead to those conspicuous results which we attribute to genius. In every branch of science, literature, art, or industry, there are thousands of men and women who work with unceasing patience, and thereby ensure at least a moderate success; but it would be absurd to assent for a moment to crude notions of human equality, and to allow that equal amounts of intellectual labour yield equal results. A Newton may modestly and sincerely attribute his discoveries to industry and patient thought, and there is much reason to believe that genius is essentially unconscious and unable to account for its own peculiar powers.

If genius, indeed, be that by which intellect diverges from what is common, it must necessarily be a phenomenon beyond the domain of the ordinary laws of nature. Nevertheless, it is always an interesting and instructive work to trace out, as far as possible, the characteristics of mind by which great discoveries have been achieved, and we shall find in the analysis much to illustrate the principles of scientific method.

Error of the Baconian Method.

Hundreds of investigators may be constantly engaged in experimental inquiry; they may compile numberless notebooks full of scientific facts, and may frame endless tables full of numerical results; but if the views of the nature of induction here maintained be true they can never by such work alone rise to new and great discoveries. By an organized system of research they may work out deductively the detailed results of a previous discovery, but to arrive at a new principle of nature is another matter. Francis Bacon contributed to spread abroad the hurtful notion that to advance science we must begin by accumulating facts, and then draw from them, by a process of patient digestion, successive laws of higher and higher generality. In protesting against the false method of the scholastic logicians, he exaggerated a partially true philosophy, until it became almost as false as that which preceded it. His notion of scientific method was that of a kind of scientific bookkeeping. Facts were to be indiscriminately gathered from every source, and posted in a kind of ledger, from which would emerge in time a clear balance of truth. It is difficult to imagine a less likely way of arriving at great discoveries.

The greater the array of facts, the less is the probability that they will by any routine system of classification or

research disclose the laws of nature they embody. Exhaustive classification in all possible orders is out of the question, because the possible orders are practically infinite in number. It is before the glance of the philosophic mind that facts must display their meaning, and fall into logical order. The natural philosopher must therefore have, in the first place, a mind of impressionable character, which is readily affected by the slightest exceptional phenomenon. His associating and identifying powers must be great, that is, a single strange fact must suggest to his mind whatever of like nature has previously come within his experience. His imagination must be active, and bring before his mind multitudes of relations in which the unexplained facts may possibly stand with regard to each other, or to more common facts. Sure and vigorous powers of deductive reasoning must then come into play, and enable him to infer what will happen under each supposed condition. Lastly, and above all, there must be the love of certainty leading him diligently and with perfect candour, to compare his speculations with the test of fact and experiment.

Freedom of Theorizing.

It would be a complete error to suppose that the great discoverer is one who seizes at once unerringly upon the truth, or has any special method of divining it. In all probability the errors of the great mind far exceed in number those of the less vigorous one. Fertility of imagination and abundance of guesses at truth are among the first requisites of discovery; but the erroneous guesses must almost of necessity be many times as numerous as those which prove well founded. The weakest analogies, the most whimsical notions, the most apparently absurd theories, may pass through the teeming brain, and no

record may remain of more than the hundredth part. There is nothing intrinsically absurd except that which proves contrary to logic and experience. The truest theories involve suppositions which are most inconceivable, and no limit can really be placed to the freedom of framing hypotheses. Kepler is an extraordinary instance to this effect. No minor laws of nature are more firmly established than those which he detected concerning the orbits and motions of planetary masses, and on these empirical laws the theory of gravitation was founded. Did we not know by his own writings the multitude of errors into which he fell, we might have imagined that he had some special faculty of seizing on the truth. But, as is well known, he was full of chimerical notions; his most favourite and long entertained theory was founded on a fanciful analogy between the planetary orbits and the regular solids. His celebrated laws were the outcome of a lifetime of speculation, for the most part vain and groundless. We know this with certainty, because he had a curious pleasure in dwelling upon erroneous and futile trains of reasoning, which most other persons carefully consign to oblivion. But Kepler's name was destined to immortality, on account of the patience with which he submitted his hypotheses to comparison with observation, the candour with which he acknowledged failure after failure, and the perseverance and ingenuity with which he renewed his attack upon the riddles of nature.

Next after Kepler perhaps Faraday is the physical philosopher who has afforded us the most important materials for gaining an insight into the progress of discovery, by recording erroneous as well as successful speculations. The recorded notions, indeed, are probably at the most a tithe of the fancies which arose in his active brain. As Faraday himself said—'The world little knows how

many of the thoughts and theories which have passed through the mind of a scientific investigator, have been crushed in silence and secresy by his own severe criticism and adverse examination; that in the most successful instances not a tenth of the suggestions, the hopes, the wishes, the preliminary conclusions have been realized.'

Nevertheless, in Faraday's researches published either in the 'Philosophical Transactions' or in minor papers, in his manuscript note-books, or in various other materials, fortunately made known in his interesting life by Dr. Bence Jones, we find invaluable lessons for the experimentalist. These writings are full of speculations which we must not judge by the light of subsequent discovery. It may even be said that Faraday sometimes committed to the printing press crude ideas which a cautious friend would have counselled him to keep back or suppress. There was occasionally even a wildness and vagueness in his notions, which in a less careful experimentalist might have been fatal to the attainment of truth. This is especially apparent in a curious paper concerning Ray-vibrations; but fortunately Faraday was fully aware of the shadowy character of his speculations, and expressed the feeling in words which must be quoted. 'I think it likely,' he says[a], 'that I have made many mistakes in the preceding pages, for even to myself my ideas on this point appear only as the shadow of a speculation, or as one of those impressions upon the mind, which are allowable for a time as guides to thought and research. He who labours in experimental inquiries knows how numerous these are, and how often their apparent fitness and beauty vanish before the progress and development of real natural truth.' If, then, the experimentalist has no royal road to the discovery of the truth, it is an interesting matter

[a] 'Experimental Researches in Chemistry and Physics,' p. 372. Philosophical Magazine, 3rd Series, May 1846, vol. xxviii. p. 350.

to consider by what logical procedure he attains the truth.

If I have taken a correct view of logical method, there is really no such thing as a distinct process of induction. The probability is infinitely small that a collection of complicated facts will fall into an arrangement capable of exhibiting directly the laws obeyed by them. The mathematician might as well expect to integrate his functions by a ballot-box, as the experimentalist to draw deep truths from haphazard trials. All induction is but the inverse application of deduction, and it is by the inexplicable mental action of a gifted mind that a multitude of heterogeneous facts are caused to range themselves in luminous order as the results of some uniformly acting law. So different, indeed, are the qualities of mind required in different branches of science that it would be absurd to attempt to give an exhaustive description of the character of mind which leads to discovery. The labours of Newton could not have been accomplished except by a mind of the utmost mathematical genius; Faraday, on the other hand, has made the most extensive and undoubted additions to human knowledge without ever passing beyond common arithmetic. I do not remember meeting in Faraday's writings with a single algebraic formula or mathematical problem of any complexity. Professor Clerk Maxwell, indeed, in the preface to his new 'Treatise on Electricity,' has strongly recommended the reading of Faraday's researches by all students of science, and has given his opinion that though Faraday seldom or never employed mathematical formulæ, his methods and conceptions were not the less mathematical in their nature[b]. I have myself protested against the prevailing confusion between a mathematical and an

b See also 'Nature,' Sept. 18, 1873; vol. viii. p. 398.

exact science [c], yet I certainly think that Faraday's experiments were for the most part purely qualitative, and that his mathematical ideas were of a rudimentary character. It is true that he could not possibly investigate such a subject as magne-crystallic action without involving himself in geometrical relations of considerable complexity. I nevertheless think that he was deficient in purely mathematical deductive power, that power which is so exclusively developed by the modern system of mathematical training at Cambridge. Faraday, for instance, was perfectly acquainted with the forms of his celebrated lines of force, but I am not aware that he ever entered into the subject of the algebraic nature of those curves, and I feel sure that he could not have explained their form as depending on the resultant attraction of all the magnetic particles acting according to general mathematical laws. There are even occasional indications that he did not understand some of the simpler mathematical doctrines of modern physical science. Although he so clearly foresaw the establishment of the unity of the physical forces, and laboured so hard with his own hands to connect gravity with the other forces, it is very doubtful whether he understood the fundamental doctrine of the conservation of energy as applied to gravitation. Thus, while Faraday was probably equal to Newton in experimental skill and deductive power as regards the invention of simple qualitative experiments, he was contrasted to him in mathematical power. These two instances are sufficient to show that minds of widely different conformation may meet with suitable regions of research. Nevertheless, there are certain common traits which we may discover in all the highest scientific minds.

[c] 'Principles of Science,' vol. i. p. 317, and 'Theory of Political Economy,' pp. 3–14.

The Newtonian Method, the True Organum.

Laplace was of opinion that the 'Principia' and the 'Opticks' of Newton furnished the best models then available of the delicate art of experimental and theoretical investigation.[c] In these, as he says, we meet with the most happy illustrations of the way in which, from a series of inductions, we may rise to the causes of phenomena, and thence descend again to all the resulting details.

The popular notion concerning Newton's discoveries is that in early life, while driven into the country by the Great Plague, a falling apple accidentally suggested to him the existence of gravitation, and that, availing himself of this hint, he was led to the discovery of the law of gravitation, the explanation of which constitutes the 'Principia.' It is difficult to imagine a more ludicrous and inadequate picture of Newton's labours and position. No originality, or at least priority, could be or was claimed by Newton as regards the discovery of the celebrated law of the inverse square, so closely associated with his name. In a well-known Scholium[d] he acknowledges that Sir Christopher Wren, Dr. Hooke, and Dr. Halley, had severally observed the accordance of Kepler's third law of motion of the planets with the principle of the inverse square.

Newton's work was really that of developing the methods of deductive reasoning and experimental verification, by which alone great hypotheses can be brought to the touch-stone of fact. Archimedes was the greatest of ancient philosophers, for he showed how mathematical theory could be wedded to physical experiments; and his works are the first true Organum. Newton is the modern

[d] 'Principia,' bk. I. Prop. iv.

Archimedes, and the 'Principia' forms the true Novum Organum of scientific method. The laws which he actually established are great, but his example of the manner of establishing them is greater still. There is hardly a progressive branch of physical and mathematical science, excepting perhaps chemistry and electricity, which has not been developed from the germs of true scientific procedure which he disclosed in the 'Principia' or the 'Opticks.' Overcome by the success of his theory of universal gravitation, we are apt to forget that in his theory of sound he originated the mathematical investigation of waves and the mutual action of particles; that in his Corpuscular theory of light, however mistaken, he first ventured to apply mathematical considerations to molecular attractions and repulsions; that in his prismatic experiments he showed how far experimental verification could be pushed; that in his examination of the coloured rings named after him, he accomplished the most remarkable instance of minute measurement yet known, a mere practical application of which by M. Fizeau was recently deemed worthy of a medal by the Royal Society. We only learn by degrees how complete was his scientific insight; a few words in his third law of motion display his acquaintance with the fundamental principles of modern thermodynamics and the conservation of energy, while manuscripts long overlooked prove that in his inquiries concerning atmospheric refraction he had overcome the main difficulties of applying theory to one of the most complex of physical problems.

After all, it is only by examining the way in which he effected discoveries, that we can rightly appreciate his greatness. The 'Principia' treats not of gravity so much as of forces in general, and the methods of reasoning about them. He investigates not one hypothesis only, but mechanical hypotheses in general. Nothing so much

strikes the reader of the work as the exhaustiveness of his treatment, and the almost infinite power of his insight. If he treats of central forces, it is not any one law of force which he discusses, but many, or almost all imaginable cases, the laws and results of each of which he sketches out in a few pregnant words. If his subject is a resisting medium, it is not air or water alone, nor any one resisting medium, but resisting media in general. We have a good example of his method in the Scholium to the twenty-second proposition of the second book, in which he runs rapidly over many possible suppositions as to the laws of the compressing forces which might conceivably act in an atmosphere of gas, a consequence being drawn from each case, and that one hypothesis ultimately selected which yields results agreeing with experiments upon the pressure and density of the terrestrial atmosphere.

Newton said that he did not frame hypotheses, but, in reality, the greater part of the 'Principia' is purely hypothetical, endless varieties of causes and laws being imagined which have no counterpart in nature. The most grotesque hypotheses of Kepler or Descartes were not more imaginary. But Newton's comprehension of logical method was perfect; no hypothesis was entertained unless it was definite in conditions, and admitted of unquestionable deductive reasoning; and the value of each hypothesis was entirely decided by the comparison of its consequences with facts. I do not entertain a doubt that the general course of his procedure is identical with that view of the nature of induction, as the inverse application of deduction, which I have advocated throughout these volumes. Francis Bacon held that science should be founded on experience, but he wholly mistook the true mode of using experience, and in attempting to apply his method he ludicrously failed. Newton did not less found his method on experience, but he seized the true method

of treating it, and applied it with a power and success never since equalled. It is wholly a mistake to say that modern science is the result of the Baconian philosophy; it is the Newtonian philosophy and the Newtonian method which have led to all the great triumphs of physical science, and I repeat that the 'Principia' forms the true 'Novum Organum.'

In bringing his theories to a decisive experimental verification, Newton showed, as a general rule, an exquisite skill and ingenuity. In his hands a few simple pieces of apparatus were made to give results involving an unsuspected depth of meaning. His most beautiful experimental inquiry was that by which he proved the differing refrangibility of rays of light. To suppose that he originally discovered the power of a prism to break up a beam of white light would be a great mistake, for he speaks of procuring a glass prism to try the *celebrated phenomena of colours*. But we certainly owe to him the theory that white light is a mixture of rays differing in refrangibility, and that lights which differ in colour, differ also in refrangibility. Other persons might have conceived this theory; in fact, any person regarding refraction as a quantitative effect, must see that different parts of the spectrum have suffered different amounts of refraction. But the power of Newton is shown in the tenacity with which he followed his theory into every consequence, and tested each result by a simple but conclusive experiment. He first shows that different coloured spots are displaced by different amounts when viewed through a prism, and that their images come to a focus at different distances from the lense, as they should do, if the refrangibility differed. After excluding by various experiments a variety of indifferent circumstances, he fixes his attention upon the question whether the rays are merely shattered, disturbed, and spread out in a chance manner,

as Grimaldi supposed, or whether there is a constant relation between the colour and the refrangibility. If Grimaldi was right, it might be expected that any part of the spectrum taken separately, and subjected to a second refraction, would suffer a new breaking up, and produce some new spectrum. Newton inferred from his own theory that a particular ray of the spectrum would have a constant refrangibility, so that a second prism would merely bend it more or less, but not further disperse it in any considerable degree. By simply cutting off most of the rays of the spectrum by a screen, and allowing the remaining narrow ray to fall on a second prism, he proved the truth of this conclusion; and then slowly turning the first prism, so as to vary the colour of the ray falling on the second one, he found that the spot of light formed by the twice-refracted ray travelled up and down, a palpable proof that the amount of refrangibility varied with the colour. For his further satisfaction, he sometimes refracted the light a third or fourth time, and he found that it might be refracted upwards or downwards or sideways, and yet for each coloured light there was a definite amount of refraction through each prism. He completes the proof by showing that the separated rays may again be gathered together into white light by an inverted prism. So that no number of refractions alters the character of the light. The conclusion thus obtained serves to explain the confusion arising in the use of a common lense; with homogeneous light he shows that there is one distinct focus, with mixed light an infinite number of foci, which prevent a clear view from being obtained at any one point.

What astonishes the reader of the 'Opticks' is the persistence with which Newton follows out the consequences of a preconceived theory, and tests the one notion by a wonderful variety of simple comparisons with fact.

It is certainly the theory which leads him to the experiments, and most of these could hardly be devised by accident. The fertility with which he invents new combinations, and foresees the results, subsequently verified, produces an invincible conviction in the reader that he has possession of the truth. Newton actually remarks that it was by mathematically determining all kinds of phenomena of colours which could be produced by refraction that he had 'invented' almost all the experiments in the book, and he promises that others who shall 'argue truly,' and try the experiments with care, will not be disappointed in the results.

The philosophic method of Huyghens was almost exactly the same as that of Newton, and Huyghens' investigation of the laws of double refraction furnishes almost equally beautiful instances of theory guiding experiment. Double refraction was first discovered by accident, so far as we know, and was described by Erasmus Bartholinus in 1669. The phenomenon then appeared to be entirely exceptional, and the laws governing the two separate paths of the refracted rays were so unapparent and complicated, that even Newton altogether misunderstood the phenomenon, and it was only at the latter end of the last century that scientific men generally began to comprehend its laws.

Nevertheless, Huyghens had, with rare genius, arrived at the true theory as early as 1678. He regarded light as an undulatory motion of some medium, and in his 'Traité de la Lumière,' he pointed out that, in ordinary refraction, the velocity of propagation of the wave is equal in all directions, so that the front of an advancing wave is spherical, and reaches equal distances in equal times. But in crystals, as he supposed, the medium would be of unequal elasticity in different directions, so that a disturbance would reach unequal distances in equal times, and the wave produced would have a spheroidal form. Huy-

ghens was not satisfied with an unverified theory. He calculated what might be expected to happen when a crystal of calc-spar was cut in various directions, and he says, 'I have examined in detail the properties of the extraordinary refraction of this crystal, to see if each phenomenon which is deduced from theory would agree with what is really observed. And this being so, it is no slight proof of the truth of our suppositions and principles; but what I am going to add here confirms them still more wonderfully; that is, the different modes of cutting this crystal, in which the surfaces produced give rise to refraction exactly such as they ought to be, and as I had foreseen them, according to the preceding theory.'

The supremacy of Newton's mistaken corpuscular doctrine of light caused the theories and experiments of Huyghens to be disregarded for more than a century; but it is not easy to imagine a more beautiful or successful application of the true method of inductive investigation, theory guiding experiment, and yet wholly relying on experiment for confirmation.

Candour and Courage of the Philosophic Mind.

Perfect readiness to reject a theory inconsistent with fact is, then, a primary requisite of the philosophical mind. But it would be a mistake to suppose that this candour has anything akin to fickleness; on the contrary, readiness to reject a false theory may be combined with a peculiar pertinacity and courage in maintaining an hypothesis as long as its falsity is not actually apparent. There must, indeed, be no prejudice or bias distorting the mind, and causing it to under-estimate or pass over the unwelcome results of experiment. There must be that scrupulous honesty and flexibility of mind, which assigns an adequate value to all

evidence; indeed the more a man loves his theory, the more scrupulous should be his attention to its faults. Nothing is more common in life than to meet with some theorist, who, by long cogitation over a single theory, has allowed it to mould his mind, and render him incapable of receiving anything but as a contribution to the truth of his one theory. A narrow and intense course of thought may sometimes lead to great results, but the adoption of a wrong theory at the outset is in such a mind irretrievable. The man of one idea has but a single chance of truth. The fertile discoverer, on the contrary, chooses between many theories, and is never wedded to any one, unless impartial and repeated comparison has convinced him of its validity. He does not choose and then compare; but he compares time after time, and then chooses.

Having once deliberately chosen, the philosopher may rightly entertain his theory with the strongest love and fidelity. He will neglect no objection; for he may chance at any time to meet a fatal one; but he will bear in mind the inconsiderable powers of the human mind compared with the tasks it has to undertake. He will see that no theory can at first be reconciled with all possible objections, simply because there may be many interfering causes, or the very consequences of the theory may have a complexity which prolonged investigation by successive generations of men may not exhaust. If then, a theory exhibit a number of very striking coincidences with fact, it must not be thrown aside until at least one *conclusive discordance* is proved, regard being had to possible error in establishing that discordance. In science and philosophy something must be risked. He who quails at the least difficulty will never establish a new truth, and it was not unphilosophic in Leslie to remark concerning his own experimental investigations into the nature of heat—

'In the course of investigation, I have found myself compelled to relinquish some preconceived notions; but I have not abandoned them hastily, nor, till after a warm and obstinate defence, I was driven from every post[e].'

Faraday's life, again, furnishes most interesting illustrations of this tenacity of the philosophical mind. Though so candid in rejecting some of his theories, there were others to which he clung through everything. One of his most favourite notions was finally realised in a brilliant discovery; another remains in doubt to the present day.

The Philosophic Character of Faraday.

In Faraday's researches concerning the connexion of magnetism and light, we find an excellent instance of the pertinacity with which a favourite theory may be held and pursued, so long as the results of experiment are simply nugatory and do not clearly negative the notions entertained. In purely quantitative questions, as we have seen, the absence of apparent effect can seldom be regarded as proving the absence of all effect. Now Faraday was convinced that some mutual relation must exist between magnetism and light. As early as 1822 he attempted to produce an effect upon a ray of polarized light, by passing it through water placed between the poles of a voltaic battery; but he was obliged to record that not the slightest effect was observable. During forty subsequent years the subject, we are told[f], rose again and again to his mind, and no failure could make him relinquish his search after this unknown relation. It was in the year 1845 that he

e 'Experimental Inquiry into the Nature of Heat.' Preface, p. xv.

f Bence Jones, 'Life of Faraday,' vol. i. p. 362.

gained the first success; on August 30th he began to work with common electricity, vainly trying glass, quartz, Iceland spar, &c. Several days of labour gave no result, yet he did not desist. Heavy glass, a transparent medium of great refractive powers, composed of borate of lead, was now tried, by being placed between the poles of a powerful electro-magnet, while a ray of polarized light was transmitted through it. When the poles of the electro-magnet were arranged in certain positions with regard to the substance under trial, no effects were apparent; but at last Faraday happened fortunately to place a piece of heavy glass so that contrary magnetic poles were on the same side, and now an effect was witnessed. The glass was found to have the power of twisting the plane of polarization of the ray of light.

All Faraday's recorded thoughts upon this great experiment are replete with curious interest. He attributes his success to the opinion, almost amounting to a conviction, that the various forms, under which the forces of matter are made manifest, have one common origin, and are so directly related and mutually dependent that they are convertible. 'This strong persuasion,' he says[g], 'extended to the powers of light, and led to many exertions having for their object the discovery of the direct relation of light and electricity. These ineffectual exertions could not remove my strong persuasion, and I have at last succeeded.' He describes the phenomenon in somewhat figurative language as *the magnetization of a ray of light*, and also as *the illumination of a magnetic curve or line of force*. He has no sooner got the effect in one case, than he proceeds, with his characteristic comprehensiveness of research, to test the existence of a like phenomenon in all the substances available. He finds that not only

g 'Life of Faraday,' vol. ii. p. 199.

heavy glass, but solids and liquids, acids and alkalis, oils, water, alcohol, ether, all possess this power; but he was not able to detect its existence in any gaseous substance. His thoughts cannot be restrained from running into curious speculations as to the possible results of the power in certain cases. 'What effect,' he says, 'does this force have in the earth where the magnetic curves of the earth traverse its substance? Also what effect in a magnet?' And then he falls upon the wholly original notion that perhaps this force tends to make iron and oxide of iron transparent, a phenomenon never previously or since observed. We can meet with nothing more instructive as to the course of mind by which great discoveries are made, than these records of Faraday's patient labours, and his varied success and failure. Nor are his unsuccessful labours upon the relation of gravity and electricity less interesting, and worthy of study.

Throughout a large part of his life, Faraday was possessed by the idea that gravity cannot be unconnected with the other forces of nature. On March 19th, 1849, he wrote in his laboratory book—'Gravity. Surely this force must be capable of an experimental relation to electricity, magnetism, and the other forces, so as to bind it up with them in reciprocal action and equivalent effect[h].' He filled twenty paragraphs or more with reflections and suggestions, as to the mode of approaching the subject by experiment. He anticipated that the approach of one body to another would develope electricity in them, or that a body falling through a conducting helix would excite a current changing in direction as the motion was reversed. '*All this is a dream*,' he remarks; 'still examine it by a few experiments. Nothing is too wonderful to be true, if it be consistent with the laws of nature;

[h] See also his more formal statement in the 'Experimental Researches in Electricity,' 24th Series, § 2702, vol. iii. p. 161.

and in such things as these, experiment is the best test of such consistency.'

He executed many difficult and tedious experiments, which are described in the 24th Series of Experimental Researches; but the result was *nil.* And yet he concludes, 'Here end my trials for the present. The results are negative; they do not shake my strong feeling of the existence of a relation between gravity and electricity, though they give no proof that such a relation exists.'

He returned to the work when he was ten years older, and in 1858–9 recorded many remarkable reflections and experiments. He was much struck by the fact that electricity is essentially a *dual force*, and it had always been a peculiar conviction of Faraday that no body could be electrified positively without some other body becoming electrified negatively; some of his researches had been simple developments of this necessary relation. But observing that between two mutually gravitating bodies there was no apparent circumstance to determine which shall be positive and which negative, he does not hesitate to call in question an old opinion. 'The evolution of *one* electricity would be a new and very remarkable thing. The idea throws a doubt on the whole; but still try, for who knows what is possible in dealing with gravity.' We cannot but notice the candour with which he thus in his laboratory book acknowledges the doubtfulness of the whole thing, and is yet prepared as a forlorn hope to frame experiments in opposition to all his previous experience of the course of nature. For a time his thoughts flow on as if the strange detection were already made, and he had only to trace out its consequences throughout the universe. 'Let us encourage ourselves by a little more imagination prior to experiment,' he says, and then he reflects upon the infinity of actions

in nature, in which the mutual relations of electricity and gravity would come into play; he pictures to himself the planets and the comets charging themselves as they approach the sun; cascades, rain, rising vapour, circulating currents of the atmosphere, the fumes of a volcano, the smoke in a chimney become so many electrical machines. A multitude of events and changes in the atmosphere seem to be at once elucidated by such actions; for a moment his reveries have the vividness of fact. 'I think we have been dull and blind not to have suspected some such results,' and he sums up rapidly the consequences of his great but imaginary theory; an entirely new mode of exciting heat or electricity, an entirely new relation of the natural forces, an analysis of gravitation, and a justification of the conservation of force. Such were Faraday's fondest dreams of what might be, and to many another philosopher they would have been a sufficient basis for the writing of a great book. But Faraday's imagination was within his full control; as he himself says, 'Let the imagination go, guarding it by judgment and principle, and holding it in and directing it by experiment.' His dreams soon took a very practical form, and for many subsequent days he laboured with ceaseless energy, on the staircase of the Royal Institution, in the clock tower of the Houses of Parliament, or in the Shot Tower at Southwark, raising and lowering heavy weights, and combining electrical helices and wires in every conceivable way. His skill and long experience in experiment were severely taxed to eliminate the effects of the earth's magnetism, and time after time he saved himself from accepting mistaken indications, which to another man might have seemed conclusive verifications of his theory. When all was done there remained absolutely no results. 'The experiments,' he says, 'were well made, but the results are negative;' and yet he adds, 'I cannot accept them as

conclusive.' In this position the question remains to the present day; it may be that the effect was too slight to be detected, or it may be that the arrangments adopted were not suited to develope the particular relation which exists, just as Oersted could not detect electro-magnetism, so long as his wire was perpendicular to the plane of motion of his needle. But these are not matters which concern us further here. We have only to notice the profound conviction in the unity of natural laws, the active powers of inference and imagination, the unbounded licence of theorizing, combined above all with the utmost diligence in experimental verification which this remarkable research manifests.

Reservation of Judgment.

There is yet another characteristic needed in the philosophic mind; it is that of suspending judgment when the data are insufficient. Many people will express a confident opinion on almost any question which is put before them, but they thereby manifest not strength, but weakness and narrowness of mind. To see all sides of a complicated subject, and to weigh all the different facts and probabilities correctly, may require no ordinary powers of comprehension. Hence it is most frequently the philosophic mind which is in doubt, and the ignorant mind which is ready with a positive decision. Faraday has himself said, in a very interesting lecture[1], 'Occasionally and frequently the exercise of the judgment ought to end in *absolute reservation.* It may be very distasteful, and great fatigue, to suspend a conclusion; but as we are not infallible, so we ought to be cautious; we shall eventually find our advantage, for the man who

[1] Printed in 'Modern Culture,' edited by Youmans, p. 219.

rests in his position is not so far from right as he who, proceeding in a wrong direction, is ever increasing his distance.'

Arago presented a conspicuous example of this high quality of mind, as Faraday remarks; for when he made known his curious discovery of the relation of a magnetic needle to a revolving copper plate, a number of supposed men of science in different countries gave immediate and confident explanations of it, which were all wrong. But Arago, who had both discovered the phenomenon and personally investigated its conditions, declined to put forward publicly any theory at all.

At the same time we must not suppose that the truly philosophic mind can tolerate a state of doubt, while a chance of decision remains open. In science nothing like compromise is possible, and truth must be one. Hence, doubt is the confession of ignorance, and must involve a painful feeling of incapacity. But doubt lies between error and truth, so that if we choose wrongly we are further away than ever from our goal.

Summing up, then, it would seem as if the mind of the great discoverer must combine almost contradictory attributes. He must be fertile in theories and hypotheses, and yet full of facts and precise results of experience. He must entertain the feeblest analogies, and the merest guesses at truth, and yet he must hold them as worthless till they are verified in experiment. When there are any grounds of probability he must hold tenaciously to an old opinion, and yet he must be prepared at any moment to relinquish it when a single clearly contradictory fact is encountered. 'The philosopher,' says Faraday[k], 'should be a man willing to listen to every suggestion, but determined to judge for himself. He should not be biassed by

k Bence Jones, 'Life of Faraday,' vol. i. p. 225.

appearances; have no favourite hypothesis; be of no school; and in doctrine have no master. He should not be a respecter of persons, but of things. Truth should be his primary object. If to these qualities be added industry, he may indeed hope to walk within the veil of the temple of nature.'

BOOK V.

GENERALIZATION, ANALOGY, AND CLASSIFICATION.

CHAPTER XXVII.

GENERALIZATION.

I HAVE endeavoured to show in preceding chapters that all inductive reasoning is an inverse application of deductive reasoning, and consists in demonstrating that the consequences of certain assumed propositions or laws agree with facts of nature gathered by active or passive observation. The fundamental process of reasoning, as stated in the outset, consists in inferring of any thing what we know of similar objects, and it is on this principle that the whole of deductive reasoning, whether simply logical or mathematico-logical, is founded. All inductive reasoning must therefore be founded on the same principle. Now it might seem that by a very plain use of this principle we might avoid the complicated processes of induction and deduction, and argue directly from one particular case to another, as the late Mr. J. S. Mill proposed. If the Earth, Venus, Mars, Jupiter, and other planets move in elliptic orbits, cannot we dispense with all elaborate precautions, and assert that Neptune, Ceres, or the last discovered planet must do so likewise? Do

we not know that Mr. Gladstone must die, because he is like other men? May we not argue that because some men die therefore he must? Is it requisite to ascend by induction to the general proposition 'all men must die,' and then descend by deduction from that general proposition to the case of Mr. Gladstone? My answer will be undoubtedly that it is necessary to ascend to general propositions. The fundamental principle of the substitution of similars gives us no warrant in affirming of Mr. Gladstone what we know of other men, simply because we cannot be sure that Mr. Gladstone is exactly similar to other men. Until his death we cannot be perfectly sure that he possesses precisely all the attributes of other men; it is a question of probability, and I have endeavoured to explain the mode in which the theory of probability is applied to calculate the probability that from a series of similar events we may infer the recurrence of like events under identical circumstances. There is then no such process as that of inferring from particulars to particulars. A careful analysis of the conditions under which such an inference appears to be made, shows that the process is really a general one, and that what is inferred of a particular case might be inferred of all similar cases. All reasoning is essentially general, and all science implies generalization. In the very birth-time of philosophy this was held to be so: 'Nulla scientia est de individuis, sed de solis universalibus,' was the doctrine of Plato, delivered by Porphyry. And Aristotle[a] held a like opinion—Οὐδεμία δὲ τέχνη σκοπεῖ τὸ καθ' ἕκαστον . . . τὸ δὲ καθ' ἕκαστον ἄπειρον, καὶ οὐκ ἐπιστητόν. 'No art treats of particular cases; for particulars are infinite and cannot be known.' No one who holds the doctrine that reasoning may be from particulars to particulars, can be supposed to have

[a] Aristotle's 'Rhetoric,' Liber I. 2. 11.

the most rudimentary notion of what constitutes reasoning and science.

At the same time there can be no doubt that practically what we find to be true of many similar objects will probably be true of the next similar object. This is the result to which an analysis of the Inverse Method of Probabilities leads us, and, in the absence of any precise data from which we may calculate probabilities, we are usually obliged to make a rough assumption that similars in some respects are similars in other respects. Thus it comes to pass that a very large part of the reasoning processes in which scientific men are engaged, seems to consist in detecting similarities between objects, and then rudely assuming that the like similarities will be detected in other cases.

Distinction of Generalization and Analogy.

There is no distinction but that of degree between what is known as reasoning by *generalization* and reasoning by *analogy*. In both cases from certain observed resemblances we infer, with more or less probability, the existence of other resemblances. In generalization the resemblances have great extension and usually little intension, whereas in analogy we rely upon the great intension, the extension being of small amount (vol. i. p. 31). If we find that the qualities A and B are associated together in a great many instances, and have never been found separate, it is highly probable that on the next occasion when we meet with A, B will also be found to be present, and *vice versâ*. Thus wherever we meet with an object possessing gravity, it is found to possess inertia also, nor have we met with any material objects possessing inertia without discovering that they also possess gravity. The probability has therefore become very great, as indicated by the rules founded

on the Inverse Method of Probabilities (vol. i. pp. 276–312), that whenever in the future we meet an object possessing either one of the properties of gravity and inertia, it will be found on examination to possess the other of these properties. This is a clear instance of the employment of generalization.

In analogy, on the other hand, we reason from likeness in many points to likeness in other points. The qualities or points of resemblance are now numerous, not the objects. At the poles of Mars are two white spots which resemble in many respects the white regions of ice and snow at the poles of the earth. There probably exist no other similar objects with which to compare these, yet the exactness of the resemblance enables us to infer, with high probability, that the spots on Mars would be found to consist of ice and snow, if we could examine them.

In short, many points of resemblance imply many more. From the appearance and behaviour of those white spots we infer that they have all the chemical and physical properties of frozen water. The inference is of course only probable, and based upon the improbability that aggregates of many qualities should be formed in a like manner in two or more cases, without being due to some single uniform condition or cause. In reasoning by analogy, then, we observe that two objects A B C D E . . . . . and A′ B′ C′ D′ E′ . . . . . have many like qualities, as indicated by the identity of the letters, and we infer that, since the first has another quality, X, we shall also discover this quality in the second case by sufficiently close examination. As Laplace says,—'Analogy is founded on the probability that similar things have causes of the same kind, and produce the same effects. The more perfect this similarity, the greater is this probability'[b]. The nature

[b] 'Essai Philosophique sur les Probabilités,' p. 86.

of analogical inference is also very correctly described in the Logic attributed to Kant, where the rule of ordinary induction is stated in the words '*Eines in vielen, also in allen,*' one quality in many things, therefore in all; and the rule of analogy is '*Vieles in einem, also auch das übrige in demselben*'[c], many (qualities) in one, therefore also the remainder in the same.

It is evident that there may be intermediate cases in which, from the resemblance of a moderate number of objects in several properties, we may infer to other objects. Probability must rest either upon the number of instances or the depth of resemblance, or upon the occurrence of both in sufficient degrees. What there is wanting in extension must be made up by intension, and *vice versâ*.

Two Meanings of Generalization.

The term generalization, as commonly used, includes two processes which are of different character, but are often closely associated together. In the first place, we generalize whenever we recognise even in two facts or objects a certain common nature. We cannot detect the slightest similarity without opening the way to inference from one case to the other. If we compare a cubical with a regular octahedral crystal, there is little apparent similarity; but, so soon as we perceive that either can be produced by the symmetrical modification of the other, we discover a groundwork of similarity in the constitution of the crystals, which enables us to infer many things of one, because they are true of the other. Our knowledge of ozone took its rise from the time when the similarity of smell, attending electric sparks, strokes of lightning, and the slow combustion of phosphorus, was noticed by

c Kant's 'Logik,' § 84, Königsberg, 1800, p. 207.

Schönbein. There was a time when the rainbow was an entirely inexplicable phenomenon, a portent, like a comet, and a cause of superstitious hopes and fears. But we find the true spirit of science in Roger Bacon, who desires us to consider the objects which present the same colours as the rainbow; he mentions hexagonal crystals from Ireland and India, but he bids us not suppose that the hexagonal form is essential, for similar colours may be detected in many other transparent stones. Drops of water scattered by the oar in the sun, the spray from a water-wheel, the dew-drops lying on the grass in the summer morning, all display a similar phenomenon[d]. No sooner have we grouped together these apparently diverse instances, than we have begun to generalize, and have acquired a power of applying to one instance what we can detect of others. Even when we do not apply the knowledge gained to new objects and phenomena, our comprehension of those already observed is vastly strengthened and deepened by thus learning to view them as particular cases of one more general property.

A second process, to which the name of generalization is equally given, consists in passing from a given fact or partial law to a multitude of unexamined cases, which we believe to be subject to the same conditions. Instead of merely recognising similarity as it is brought before us, we predict its existence before our senses can detect it, so that generalization of this kind endows us with a prophetic power of more or less probability. Having observed that many substances assume, like water and mercury, the three states of solid, liquid, and gas, and having assured ourselves by frequent trial that the greater the means we possess of heating or cooling, the more substances we can vapourize and freeze, we pass confidently

[d] Whewell's 'Philosophy of the Inductive Sciences,' 2nd edit. vol. ii. p. 171, quoting the 'Opus Majus,' p. 473.

in advance of fact, and assume that all substances are capable of these three forms. Such a generalization was accepted by men of the high intellect of Lavoisier[e] and Laplace[f] before many of the corroborative facts now in our possession were known. The reduction of a single comet beneath the sway of gravity was at once considered sufficient indication that all comets must obey the same power. Few persons doubted that the same great law extended over the whole heavens; certainly the fact that a few stars out of many millions make manifest the action of gravity, is now held to be sufficient evidence to establish the general extension of the laws of Newton over the sphere of the visible universe.

Value of Generalization.

It might seem that if we know particular facts, there can be little use in connecting them together by a general law. The particulars must be more full of useful information than an abstract general statement. If we know, for instance, the properties of an ellipse, a circle, a parabola, and hyperbola, what is the use of learning all these properties over again in the general theory of curves of the second degree? If we understand the phenomena of sound and light and water-waves separately, what is the need of erecting a general theory of waves, which, after all, is inapplicable to practice until resolved into particular cases again? But, in reality, we never do obtain an adequate knowledge of particulars until we regard them as cases of the general. Not only is there a singular delight in discovering the many in the one, and the one in the many, but there is a constant interchange of light and knowledge.

e 'Chemistry,' translated by Kerr, 3rd edit. pp. 63, 77.

f 'System of the World,' ditto vol. i. p. 202.

Properties which are unapparent in the hyperbola may readily be discovered in the ellipse. Most of the complex relations which the old geometers discovered in the circle will be reproduced *mutatis mutandis* in the other conic sections. The undulatory theory of light might have been unknown at the present day, had not the theory of sound supplied hints by analogy. The study of light has made known many phenomena of interference and polarization, the existence of which had hardly been suspected in the case of sound, but which may now be sought out, and perhaps found to possess unexpected interest and importance. The careful study of water-waves shows how waves may alter in form and velocity with varying depth of water. Analogous changes may sometimes be detected in sound waves. Thus there is a mutual interchange of aid.

'Every study of a generalization or extension,' as De Morgan has well said[g], 'gives additional power over the particular form by which the generalization is suggested. Nobody who has ever returned to quadratic equations after the study of equations of all degrees, or who has done the like, will deny my assertion that οὐ βλέπει βλέπων may be predicated of any one who studies a branch or a case, without afterwards making it part of a larger whole. Accordingly it is always worth while to generalize, were it only to give power over the *particular*. This principle, of daily familiarity to the mathematician, is almost unknown to the logician.'

Comparative Generality of Physical Properties.

Much of the value of science depends upon the knowledge which we gradually acquire of the different degrees of generality of properties and phenomena of various kinds.

[g] 'Syllabus of a proposed System of Logic,' p. 34.

The very use of science consists in enabling us to act with confidence, because we can foresee the result. Now this foresight must rest upon the knowledge of the powers which will come into play. That knowledge, indeed, can never be certain, because it rests upon imperfect induction, and the most confident beliefs and predictions of the physicist may be falsified. Nevertheless, if we always estimate the probability of each belief according to the due teaching of the data, and bear in mind that probability when forming our anticipations, we shall ensure the minimum of disappointment. Even when he cannot exactly apply the theory of probabilities, the physicist may acquire the habit of making judgments in general agreement with its principles and results.

Such is the constitution of nature, that the physicist soon learns to distinguish those properties which have wide and uniform extension, from those which vary between case and case. Not only are certain laws distinctly laid down, with their extension carefully defined, but a scientific training gives a kind of tact in judging how far other laws are likely to apply under any particular circumstances. We learn by degrees that crystals exhibit phenomena depending upon the directions of the axes of elasticity, which we must not expect in uniform solids. Liquids, compared even with non-crystalline solids, exhibit laws of far less complexity and variety; and gases assume, in many respects, an aspect of nearly complete uniformity. To trace out the branches of science in which varying degrees of generality prevail, would be found to be an inquiry of great interest and importance; but want of space, if there were no other reason, would forbid me to attempt it, except in a very slight manner.

Gases, so far as they are really gaseous, not only have exactly the same properties in all directions of space, but one gas exactly resembles other gases in a great many qualities.

All gases expand by heat, according to the one same law, and by nearly the same amount; the specific heats of equivalent weights are equal, and the densities, though not the same, are exactly proportional to the atomic weights. All such gases obey the general law, that the volume multiplied by the pressure, and divided by the absolute temperature, is constant or nearly so. The laws of diffusion and transpiration are the same in all cases, and, generally speaking, all physical laws, as distinguished from chemical laws, which apply to one gas apply equally to all other gases. Even when gases differ in chemical or physical properties, the differences are minor in degree or number. Thus the differences of viscosity are far less marked than in the liquid and solid states. Nearly all gases, again, are colourless, the exceptions being chlorine, the vapours of iodine, bromine, and some other substances.

Only in one single point, so far as I am aware, do gases present distinguishing marks unknown, or nearly so, in the solid and liquid states. I mean as regards the light given off when incandescent. Each gas, when sufficiently heated, yields its own peculiar series of rays, arising from the free vibrations of the constituent parts of the molecules when pursuing separate paths. Hence the possibility of distinguishing gases by the spectroscope. But the molecules of solids and liquids appear to be continually in conflict with each other, so that only a confused *noise* of atoms is produced, instead of a definite series of luminous chords. At the same temperature, accordingly, all solids and liquids give off nearly the same rays when strongly heated, and we have in this case a single exception to the general rule of the greater generality of properties in gases.

Liquids are in many ways intermediate in character between gases and solids. While incapable of possessing

different elasticity in different directions, and thus denuded of the rich geometrical complexity of solids, they retain the variety of density, colour, degrees of transparency, great diversity in surface tension, viscosity, coefficients of expansion, compressibility, and many other properties which we observe in solids, but not for the most part in gases. Though our knowledge of the physical properties of liquids is thus much wanting in generality at present, there is ground to hope that by degrees laws connecting and explaining the varieties of character may be traced out. Liquids ought to be compared together, not at uniform temperatures, but at points of temperature similarly related to the points of fusion and ebullition.

Solids are in every way contrasted to gases. Each solid substance has its own peculiar density, hardness, compressibility, degree of transparency, tenacity, elasticity, power of conducting heat and electricity, magnetic properties, capability of producing frictional electricity, and so forth. Even different specimens of the same kind of substance will be widely different, according to the accidental treatment it has received. And not only has each substance its own specific properties, but, when crystallized, its own properties peculiar to each direction, regard being had to the axes of crystallization. The velocity of radiation, the rate of conduction of heat, the coefficients of expansibility and compressibility, the thermo-electric properties, all vary in different crystallographic directions.

It is highly probable that many apparent differences among liquids, and even among solids, will be resolved and explained, when we learn to regard them under exactly corresponding circumstances. The extreme generality of the properties of gases is really only true at an infinitely high temperature, when they are all equally

remote from their condensing points. Now, it is found that if we compare liquids—for instance, different kinds of alcohols—not at equal temperatures, but at points equally distant from their respective boiling-points, the laws and coefficients of expansion are nearly equal. The vapour-tensions of liquids also are much more nearly equal, when thus compared at corresponding points, and the boiling-points themselves appear to be simply related to the chemical composition in many cases. No doubt the progress of investigation will often enable us to discover generality, where we at present only see variety and puzzling complexity.

In some cases substances exhibit the same physical properties in the liquid as in the solid state. Lead has a high refractive power, whether in solution, or in solid salts, crystallized, or vitreous. The magnetic power of iron is conspicuous, whatever be its chemical condition; indeed, the magnetic properties of substances, though varying with temperature, seem not to be greatly affected by physical changes. Colour, absorptive power for heat or light rays, and a few other properties are also often the same both in liquids and gases. Iodine and bromine possess a deep colour whenever they are chemically uncombined. Nevertheless, we can seldom argue safely from the properties of a substance in one condition to that in another condition. Ice is an insulator, water a conductor of electricity, and the same contrast exists in most other substances. The conducting power of a liquid for electricity increases with the temperature, while that of a solid decreases. By degrees we may learn to distinguish between those properties of matter which depend upon the intimate construction of the chemical molecule, and those which depend upon the contact, conflict, mutual attraction, or other relations of distinct molecules. The properties of a substance with respect to light seem gene-

rally to depend upon the molecule; thus, the power of certain substances to cause the plane of polarization of a ray of light to rotate, is exactly the same whatever be its degree of density, or the diluteness of the solution in which it is contained. Taken as a whole, the physical properties of substances and their quantitative laws, present a problem of infinite complexity, and centuries must elapse before any moderately complete generalizations on the subject become possible.

Uniform Properties of all Matter.

Some laws are held to be true of all matter in the universe absolutely, without exception, no instance to the contrary having ever been noticed. This is the case with the laws of motion, as laid down by Galileo and Newton. It is also conspicuously true of the law of universal gravitation. The rise of modern physical science may perhaps be considered as beginning at the time when Galileo showed, in opposition to the Aristotelians, that matter is equally affected by gravity, irrespective of its form, magnitude, or texture. All objects fall with equal rapidity, when disturbing causes, such as the resistance of the air, are removed or allowed for. That which was rudely demonstrated by Galileo from the leaning tower of Pisa, was proved by Newton to a high degree of approximation, in an experiment which has already been referred to (vol. ii. p. 55).

Newton formed two pendulums of as nearly as possible similar outward shape, by taking two equal round wooden boxes, and suspending them by equal threads, eleven feet long. The motion of each pendulum was therefore equally subject to the resistance of the air. He filled one box with wood, and in the centre of oscillation of the

other placed an equal weight of gold. The pendulums were then equal in weight and in size; and, on setting them simultaneously in motion, Newton found that they vibrated for a great length of time with exactly equal vibrations. He tried the same experiment with silver, lead, glass, sand, common salt, water, and wheat, instead of gold, and ascertained that the rapidity of motion of his pendulum was exactly the same whatever was the kind of matter inside them [h]. He considered that a difference of a thousandth part would have been apparent. The reader must observe that the pendulums were made of equal weight only in order that they might suffer equal retardation from the air. The meaning of the experiment is that all the substances manifest exactly equal acceleration from the force of gravity, and that therefore the inertia or resistance of matter to force, which is the only independent measure of mass in our possession, is always proportional to gravity.

These experiments of Newton were considered conclusive up to very recent times, when certain discordances between the theory and observations of the movements of planets led Nicolai, in 1826, to suggest that the equal gravitation of different kinds of matter might not be absolutely exact. It is perfectly philosophical and desirable thus to call in question, from time to time, some of the best accepted laws. On this occasion Bessel carefully repeated the experiments of Newton with pendulums composed of ivory, glass, marble, quartz, meteoric stones, &c., but was unable to detect the least difference. This conclusion is also confirmed by the ultimate agreement of all the calculations of physical astronomy based upon it. Thus, whether the mass of Jupiter be calculated from the motion of its own satellites, from the effect upon the small

[h] 'Principia,' bk. III. Prop. VI. Motte's translation, vol. ii. p. 220.

planets, Vesta, Juno, &c., or from the perturbation of Encke's Comet, the results are closely accordant, showing that precisely the same law of gravity applies to the most different bodies which we can observe. The gravity or weight of a body, again, appears to be entirely independent of its other physical conditions, being totally unaffected by any alteration in the temperature, density, electric or magnetic condition, or other physical properties of the substance.

One almost paradoxical result of the law of equal gravitation is the theorem of Torricelli, to the effect that all liquids of whatever density fall or flow with equal rapidity. If there be two equal cisterns respectively filled with mercury and water, the mercury, though thirteen times as heavy, would flow from an aperture neither more rapidly nor more slowly than the water, and the same would be true of ether, alcohol, or any other liquids, allowance being made for the resistance of the air, and the differing viscosities of the liquids.

In its exact equality and its perfect independence of every circumstance, except mass and distance, the force of gravity stands apart from all the other forces and phenomena of nature, and has not yet been brought into any relation with them except through the general principle of the conservation of energy. Magnetic attraction, as remarked by Newton, follows a wholly different law as depending upon the chemical quality and molecular structure of each particular substance.

We must remember that in saying 'all matter gravitates,' we exclude from the term matter the basis of light-undulations, which is almost infinitely more extensive in amount, and obeys in many other respects the laws of mechanics. This adamantine basis of undulations appears, so far as can be ascertained, to be perfectly uniform in its properties when existing in space unoccupied by matter.

Light and heat are conveyed by it with equal velocity in all directions, and in all parts of space so far as observation informs us. But the presence of gravitating matter modifies the density and mechanical properties of the so-called ether in a way which is yet quite unexplained.

Leaving gravity, it is somewhat difficult to discover other laws which are equally true of all matter. Boerhaave was considered to have established that all bodies expand by heat, but not only is the expansion very different in different substances, but we now know positive exceptions. Many liquids and a few solids contract by heat at certain temperatures. There are indeed other relations of heat to matter which seem to be universal and uniform; thus all substances begin to give off rays of heat or light at the same temperature, according to the law of Draper; and gases will not be an exception if sufficiently condensed, as in the experiments of Frankland. Grove considers it to be universally true that all bodies in combining produce heat; all solids, with the doubtful exception of sulphur and selenium, in becoming liquid, and all liquids in becoming gases, absorb a certain quantity of heat; but the quantities of heat absorbed vary with the chemical qualities of the matter. On the other hand, Carnot's Thermodynamic Law is held to be exactly true of all matter without distinction; it expresses the fact that the amount of mechanical energy which might be theoretically obtained from a certain amount of heat energy depends only upon the temperatures between which a substance is made to change, so that whether an engine be worked by water, air, alcohol, ammonia, or any other substance, the result would theoretically be the same, if the boiler and condenser were employed at similar temperatures.

Variable Properties of Matter.

I have enumerated some of the few properties of matter, which are manifested in exactly the same manner by all substances, whatever be their differences of chemical or physical constitution. But by far the greater number of qualities vary in degree; substances are more or less dense, more or less transparent, more or less compressible, more or less magnetic, and so on. One very common result of the progress of science is to show that qualities supposed to be entirely absent from many substances are present only in so low a degree of intensity that the means of detection were insufficient. Newton believed that most bodies were not affected by the magnet at all; Faraday and Tyndall have rendered it very doubtful whether any substance whatever is wholly non-magnetic, including under that term diamagnetic properties. We are rapidly learning to believe that there are no substances absolutely opaque, or non-conducting, non-electric, non-elastic, non-viscous, non-compressible, insoluble, infusible, or non-volatile. All tends to become a matter of degree, or sometimes of direction. There may be some substances oppositely affected to others, as ferro-magnetic substances are oppositely affected to diamagnetics, or as substances which contract by heat are opposed to those which expand; but the tendency is certainly for every affection of one kind of matter to be represented by something similar in other kinds. On this account one of Newton's rules of philosophizing seems quite to lose all validity; he said, 'Those qualities of bodies which are not capable of being heightened and remitted, and which are found in all bodies on which experiment can be made, must be considered as universal qualities of all bodies.' As far as I can see, the contrary is more probable, namely,

that qualities variable in degree will be found in every substance in a greater or less degree.

It is highly remarkable that Newton, whose method of investigation was logically perfect, seemed incapable of generalizing and describing his own procedure. His celebrated 'Rules of reasoning in Philosophy,' described at the commencement of the third book of the 'Principia,' are of very questionable truth, and still more questionable value.

Extreme Instances of Properties.

Although, as we have seen, substances usually differ only in degree as regards their physical properties, great interest may attach to particular substances which manifest a property in a conspicuous and intense manner. Every branch of physical science has usually been developed from the attention forcibly drawn to some singular substance. Just as the loadstone disclosed magnetism and amber frictional electricity, so did Iceland spar point out the existence of double refraction, and sulphate of quinine the phenomenon of fluorescence. When one such startling instance has drawn the attention of the scientific world, numerous less remarkable cases of the phenomenon will soon be detected, and it will probably prove that the property in question is actually universal to all matter. Nevertheless, the extreme instances retain their interest, partly in a historical point of view, partly because they furnish the most convenient substances for experiment.

Francis Bacon was fully aware of the value of such examples, which he called *Ostensive Instances* or Light-giving, Free or Predominant Instances. 'They are those,' he says, 'which show the nature under investigation naked, in an exalted condition, or in the highest degree

of power; freed from impediments, or at least by its strength predominating over and suppressing them[i].' He mentions quicksilver as an ostensive instance of weight or density, thinking it not much less dense than gold, and more remarkable than gold as joining density to liquidity. The magnet is mentioned as an ostensive instance of attraction[k]. It would not be very easy to distinguish clearly between these ostensive instances and those which he calls *Instantiae Monodicae*, or *Irregulares*, or *Heteroclitae*, under which he places whatever is extravagant in its properties or magnitude, or exhibits least similarity to other things, such as the sun and moon among the heavenly bodies, the elephant among animals, the letter *s* among letters, or the magnet among stones[l].

In optical science great use has been made of the high dispersive power of the transparent compounds of lead, that is, the power of giving a long spectrum (vol. i. p. 32). Dolland having noticed the peculiar dispersive power of lenses made of flint-glass employed them to produce an achromatic arrangement. The element strontium presents a contrast to lead in this respect, being characterized by a remarkably low dispersive power; but I am not aware that this property has yet been turned to account.

Compounds of lead have both a high dispersive and a high refractive index, and in the latter respect they proved very useful to Faraday. Having spent much labour in preparing various kinds of optical glass, Faraday happened to form a compound of lead, silica, and boracic acid, now known as *heavy glass*, which possessed an intensely high refracting power. Many years afterwards in attempting to discover the action of magnetism upon light he failed to detect any effect, as has been

[i] 'Novum Organum,' bk. II. Aphorism 24.
[k] Ibid. Aphorism 25.
[l] Ibid. Aphorism 28.

already mentioned (vol. ii. p. 235), until he happened to test a piece of the heavy glass. The peculiar refractive power of this medium caused the magnetic strain to be apparent, and the rotation of the plane of polarization was discovered.

In almost every other part of physical science there is some substance of powers pre-eminent for the special purpose to which it is put. Rock-salt is invaluable for its extreme diathermancy or transparency to the least refrangible rays of the spectrum. Quartz is equally valuable for its transparency, as regards the ultra-violet or most refrangible rays. Diamond is the most highly refracting substance which is at the same time transparent; were it more abundant and easily worked it would be of great optical importance. Cinnabar is distinguished by possessing a power of rotating the plane of polarization of light, from 15 to 17 times that of quartz. In electric experiments copper is employed for its high conducting powers and exceedingly low magnetic properties; iron is of course essential for its enormous and almost anomalous magnetic powers; while bismuth holds a like place as regards its diamagnetic powers, and was of much importance in Tyndall's decisive researches upon the polar character of the diamagnetic force[m]. In regard to magne-crystallic action the mineral cyanite is highly remarkable, being so powerfully affected by the earth's magnetism, that when delicately suspended, it will assume a constant position with regard to the magnetic meridian, and may almost be used like the compass needle. Sodium is distinguished by its unique light-giving powers, which are so extreme that probably one half of the whole number of stars in the heavens have a yellow tinge in consequence.

It is highly remarkable that water, though the most common of all fluids, is distinguished in almost every

[m] 'Philosophical Transactions,' (1856), vol. cxlvi. p. 246.

respect by the most marked qualities. Of all known substances water has the highest specific heat, being thus peculiarly fitted for the purpose of warming and cooling, to which it is often put. It rises by capillary attraction to a height more than twice that of any other liquid. In the state of ice it is nearly twice as dilatable by heat as any other known solid substance[n]. In proportion to its density it has a far higher surface tension than any other substance, being in fact surpassed in absolute tension only by mercury, and it would not be difficult to extend considerably the list of its remarkable and useful properties.

Under extreme instances we may include cases of remarkably low powers or qualities, equally with those of the opposite extreme. Such cases seem to correspond to what Bacon calls *Clandestine Instances*, which exhibit a given nature in the least intensity, and as it were in a rudimentary state[o]. They may often be important, he thinks, as allowing the detection of the cause of the property by difference. I may add that in some cases they may be of use in experiments. Thus hydrogen is at once the least dense of all known substances, and has the least atomic weight. Liquefied nitrous oxide has the lowest refractive index of all known fluids[p]. The compounds of strontium have the lowest dispersive powers on light. It will be obvious that a property of very low degree may prove as curious and valuable a phenomenon as a property of very high degree.

The Detection of Continuity.

We should always bear in mind that phenomena which are in reality of a closely similar or even identical nature,

n 'Philosophical Magazine,' 4th Series, January 1870, vol. xxxix. p. 2.

o 'Novum Organum,' bk. II. Aphorism 25.

p Faraday's 'Experimental Researches in Chemistry and Physics,' p. 93.

may present to the senses very different appearances. Without a careful analysis of the changes which take place, we may often be in danger of widely separating facts and processes, which are actually instances of the same law. Extreme difference of degree or magnitude is a very frequent cause of error. It is truly difficult at the first moment to recognise any similarity between the gradual rusting of a piece of iron, and the rapid combustion of a heap of straw. Yet Lavoisier's chemical theory was founded upon the close similarity of the oxydizing process in one case and the other. We have only indeed to divide the iron into excessively small particles to discover that it is really the more combustible of the two, so that it actually takes fire spontaneously and burns like tinder. It is the excessive slowness of the process in the case of a massive piece of iron which disguises its real character.

If Xenophon reports truly, Socrates was seriously misled by not making sufficient allowance for extreme differences of degree and quantity. He rejected the acute opinion of Anaxagoras that the sun is a fire, on the ground that we can look at a fire, but not at the sun, and that plants grow by sunshine while they are killed by fire. He also pointed out that a stone heated in a fire is not luminous, and soon cools, whereas the sun ever remains equally luminous and hot[q]. All such mistakes evidently arise from not perceiving that difference of quantity may be so extreme as to assume the appearance of difference of quality. It is the least creditable thing we know of Socrates, that when pointing out these supposed mistakes of earlier philosophers, he advised his followers not to study astronomy.

Masses of matter of very different size may always be

[q] 'Memorabilia,' iv. 7; quoted by Whewell, 'History of Inductive Sciences,' vol. i. p. 340.

expected to exhibit great apparent differences of conduct, arising simply from the very various intensity of the forces brought into play. Many persons have thought it requisite to imagine occult forces producing the suspension of the clouds, and there have even been absurd theories representing cloud particles as minute water-balloons buoyed up by the warm air within them. But we have only to take proper account of the enormous comparative resistance which the air opposes to the fall of minute particles, to see that all cloud particles are probably constantly falling through the air, but so slowly that there is no apparent effect. Mineral matter again is always regarded as inert and incapable of spontaneous movement. We are struck by astonishment on observing in a powerful microscope, that every kind of solid matter suspended in extremely minute particles in pure water, acquires an oscillatory movement, often so marked as to resemble dancing or skipping. I conceive that this movement is entirely due to the vast comparative intensity of chemical actions when exerted upon minute particles, the effect being 5000 or 10,000 greater in proportion to the mass than in fragments of an inch diameter (vol. ii. p. 9).

Much that was formerly obscure in the science of electricity, arose from the extreme differences of intensity and quantity in which this form of energy manifests itself. Between the instantaneous and brilliant discharge of a thunder-cloud and the gentle continuous current produced by two pieces of metal and some dilute acid, there was no apparent analogy whatever. It was therefore a work of great importance when Faraday demonstrated the identity of the forces in action, showing that common frictional electricity would decompose water like that from the voltaic battery. The relation of the phenomena became plain when he succeeded in showing that it would require 800,000 discharges of his large Leyden battery to

decompose one single grain of water. Lightning was now seen to be electricity of excessively high tension, but extremely small quantity, the difference being somewhat analogous to that between the force of one million gallons of water falling through one foot, and one gallon of water falling through one million feet. Faraday estimated that one grain of water acting on four grains of zinc, would yield electricity enough for a great thunderstorm.

It was long believed that electrical conductors and insulators belonged to two opposed classes of substances. Between the inconceivable rapidity with which the current passes through pure copper wire, and the apparently complete manner in which it is stopped by a thin partition of gutta-percha or gum-lac, there seemed to be no resemblance. Faraday, again, laboured successfully to show that these were but the extreme cases of a chain of substances varying in all degrees in their powers of conduction. Even the best conductors, such as pure copper or silver offer some resistance to the electric current. The other metals have considerably higher powers of resistance, and we pass gradually down through oxides and sulphides. The best insulators, on the other hand, allow of an atomic induction which is the necessary antecedent of conduction. Hence Faraday inferred that whether we can measure the effect or not, all substances discharge electricity more or less[r]. One consequence of this doctrine must be, that every discharge of electricity produces an induced current. In the case of the common galvanic current we can readily detect the induced current in any parallel wire or other neighbouring conductor, and can separate the opposite currents which arise at the moments when the original currents begin and end. But a discharge of high tension electricity like lightning, though it certainly occupies time and has a beginning and an end,

r 'Experimental Researches in Electricity,' Series xii. vol. i. p. 420.

yet lasts so minute a fraction of a second, that it would be hopeless to attempt to detect and separate the two opposite induced currents, which are nearly simultaneous and exactly neutralise each other. Thus an apparent failure of analogy is explained away, and we are furnished with another instance of a phenomenon incapable of observation and yet theoretically known to exist[8].

Perhaps the most extraordinary and fundamental case of the detection of unsuspected continuity is found in the discovery of Cagniard de la Tour and Professor Andrews, that the liquid and gaseous conditions of matter are only remote points in a continuous course of change. Nothing is at first sight more apparently distinct than the physical condition of water and aqueous vapour. At the boiling-point there is an entire breach of continuity, and the gas produced is subject to laws incomparably more simple than the liquid from which it arose. But Cagniard de la Tour showed that if we maintain a liquid under sufficient pressure its boiling point may be indefinitely raised, and yet the liquid will ultimately assume the gaseous condition with but a small increase of volume. Professor Andrews, recently following out a similar course of inquiry, has shown that liquid carbonic acid may, at a particular temperature (30°·92 C.), and under the pressure of 74° atmosphere, be at once in a state indistinguishable from that of liquid and gas. At higher pressures carbonic acid may be made to pass from a palpably liquid state to a truly gaseous state without any abrupt change whatever. The subject is one of some complexity, because as the pressure is greater the abruptness of the change from liquid to gas gradually decreases, and finally vanishes. As similar phenomena or an approximation to them have been observed in various other liquids, there is little doubt that we may make a wide generalization,

[8] 'Life of Faraday,' vol ii. p. 7.

and assert that, under adequate pressure, every liquid might be made to pass into a gas without any breach of continuity[t].

The liquid state, again, is considered by Professor Andrews to be but an intermediate step between the solid and gaseous conditions. There are various indications that the process of melting is not perfectly abrupt; and could the experiments be made under adequate pressures, it is believed that every solid could be made to pass by insensible degrees into the state of liquid, and subsequently into that of gas.

These discoveries appear to open the way to most important and fundamental generalizations, but it is probable that in many other cases phenomena now regarded as discrete may be shown to be different degrees of the same process. The late Professor Graham was of opinion that chemical affinity differed but in degree from the ordinary attraction which holds different particles of a body together. He found that sulphuric acid continued to evolve heat when mixed even with the fiftieth equivalent of water that is added to it, so that there seemed to be no distinct limit to chemical affinity. He concludes—'There is reason to believe that chemical affinity passes in its lowest degree into the attraction of aggregation'[u].

The atomic theory is well established, but its limits are not marked out. As Mr. Justice Grove suggests, we may by selecting sufficiently high multipliers express any combination or mixture whatever of elements in terms of their equivalent weights[x]. Sir W. Thompson has suggested that the power which vegetable fibre, oatmeal, and many other substances possess of attracting and condensing aqueous vapour is probably continuous, or, in fact, iden-

[t] 'Nature,' vol. ii. p. 278.

[u] 'Journal of the Chemical Society,' vol. viii. p. 51.

[x] 'Correlation of Physical Forces,' 3rd edit. p. 184.

tical with capillary attraction, which is capable of interfering with the pressure of aqueous vapour and aiding its condensation[y]. There are many cases of so-called catalytic or surface action, such as the extraordinary power of animal charcoal for attracting organic matter, or of spongy platinum for condensing hydrogen, which can only be considered as exalted cases of a much more general power of attraction. The number of substances which are decomposed by light in a striking manner is very limited; but many other substances, such as vegetable colours, are affected by long exposure; on the principle of continuity we might well expect to find that all kinds of matter are more or less susceptible of change by the incidence of light rays[z]. It is the opinion of Mr. Justice Grove that wherever an electric current passes there is a tendency to decomposition, a strain on the molecules, which when sufficiently intense leads to disruption. Even a metallic conducting wire may be regarded as tending to decomposition. Davy was probably correct in describing electricity as chemical affinity acting on masses, or rather, as Grove suggests, creating a disturbance through a chain of particles[a]. Laplace went so far as to suggest that all chemical phenomena may be regarded as the results of the Newtonian law of attraction, applied to atoms of various mass and position; but the time is probably long distant when the progress of molecular philosophy and of mathematical methods will enable such a generalization to be verified or refuted.

The Law of Continuity.

Under the title Law of Continuity we may place many applications of the general principle of reasoning, that

y 'Philosophical Magazine,' 4th Series, vol. xlii. p. 451.

z Grove, 'Correlation of Physical Forces,' 3rd edit. p. 118.

a Ibid. pp. 166, 199, &c.

what is true of one case will be true of similar cases, and probably true of what are probably similar. Whenever we find that a law or similarity is rigorously fulfilled up to a certain point in time or space, we expect with a very high degree of probability that it will continue to be fulfilled at least a little longer. If we see part of a circle, we naturally expect that the form of the line will be maintained in the part hidden from us. If a body has moved uniformly over a certain space, we expect that it will continue to move uniformly. The ground of such inference is doubtless identical with that of all other inductive inferences. In continuous motion every infinitely small space passed over constitutes a separate constituent fact, and had we perfect powers of observation the smallest finite motion would include an infinity of information, which, by the principles of the inverse method of probabilities, would enable us to infer with actual certainty to the next infinitely small portion of the path. But when we attempt to infer from one finite portion of a path to another finite part, the inference will be only more or less probable, according to the comparative lengths of the parts and the accuracy of the observations; the longer our experience is, the more probable our inferences will be; the greater the length of time or space over which the inference extends, the less probable.

This principle of continuity presents itself in nature in a great variety of forms and cases. It is familiarly expressed in the dictum *Natura non agit per saltum*, in other words, no change in a natural phenomenon comes on with perfect suddenness or abruptness. There is always some notice—some forewarning of every phenomenon, and every change begins by insensible degrees, could we observe it with perfect accuracy. The cannon ball, indeed, is forced from the cannon in an inappreciable portion of time; the trigger is pulled, the fuze fired, the powder inflamed, the

ball expelled, all simultaneously to our senses. But there is no doubt that time is occupied by every part of the process, and that the ball begins to move at first with indefinite slowness. Captain Noble is able to measure by his chronoscope the progress of the shot in a 300-pounder gun, and finds that the whole motion within the barrel takes something less than $\frac{1}{200}$ of a second. It is an invariable principle of nature that no finite force can produce motion, except in a finite space of time. The amount of momentum communicated to a body is proportional to the accelerating force multiplied by the time through which it acts uniformly. Thus a slight force produces a great velocity only by long continued action. In a powerful shock, like that of a railway collision, the stroke of a hammer on a hard anvil, or the discharge of a gun, the time is very short, and therefore the accelerating forces brought into play are exceedingly great, but never infinite. In the case of a large gun the powder in exploding is said to exert for a moment a force equivalent to at least 2,800,000 horses.

Our belief in some of the most fundamental laws of nature rests upon the principles of continuity. Galileo is held to be the first philosopher who consciously employed this principle in his arguments concerning the nature of motion, and it is certain that we can never by pure experience assure ourselves of the truth even of the first law of motion. *A material particle,* we are told, *when not acted on by extraneous forces will continue in the same state of rest or motion.* This may be true, but as we can find no body which is free from the action of extraneous causes, how are we to prove it? Only by observing that the less the amount of those forces the more nearly is the law found to be true. A ball rolled along rough ground is soon stopped; along a smooth pavement it continues

longer in movement. A delicately suspended pendulum is almost free from friction against its supports, but it is gradually stopped by the resistance of the air; place it in the vacuous receiver of an air-pump and we find the motion immensely prolonged. A large planet like Jupiter experiences almost infinitely less friction, in comparison to its vast momentum, than we can produce experimentally, and we find through centuries that there is not the least evidence of the falsity of the law. Experience, then, informs us that we may approximate indefinitely to a uniform motion by sufficiently decreasing the disturbing forces. It is a pure act of inference which enables us to travel on beyond experience, and assert that, in the total absence of any extraneous force, motion would be absolutely uniform. The state of rest, again, is but a singular case in which motion is infinitely small or zero, to which we may attain, on the principle of continuity, by considering successively cases of slower and slower motion.

There are many interesting cases of physical phenomena, in which, by gradually passing from the apparent to the obscure, we can assure ourselves of the nature of phenomena which would otherwise be a matter of great doubt. Thus we can sufficiently prove, in the manner of Galileo, that a musical sound consists of rapid uniform pulses, by causing strokes to be made at intervals which we gradually diminish until the separate strokes coalesce into a uniform hum or note. With great advantage we approach, as Tyndall says, the sonorous through the grossly mechanical. In listening to a great organ we cannot fail to perceive that the longest pipes, or their partial tones, produce a tremor and fluttering of the building. At the other extremity of the scale, there is no fixed limit to the acuteness of sounds which we can hear; some individuals can hear sounds too shrill for other ears, and as there is nothing in the nature of the

atmosphere to prevent the existence of undulations incomparably more rapid than any of which we are conscious, we may infer, by the principle of continuity, that such undulations probably exist.

There are many habitual actions which we perform we know not how. So rapidly are many acts of mind accomplished that analysis seems impossible. We can only investigate them when in process of formation, observing that the best formed habit or instinct is slowly and continuously acquired, and it is in the early stages that we can perceive the rationale of the process.

Let it be observed that this principle of continuity must be held of much weight only in exact physical laws, those which doubtless repose ultimately upon the simple laws of motion. If we fearlessly apply the principle to all kinds of phenomena, we may often be right in our inference, but also often wrong. Thus, before the development of spectrum analysis, astronomers had observed that the more they increased the powers of their telescopes the more nebulæ they could resolve into distinct stars. This result had been so often found true that they almost irresistibly assumed that all the nebulæ would be ultimately resolved by telescopes of sufficient power; yet Mr. Huggins has in recent years proved by the spectroscope, that certain nebulæ are actually gaseous, and in a truly nebulous state. Even one such observation is a real exception sufficient to invalidate previous inferences as to the constitution of the universe.

The principle of continuity must have been continually employed in the inquiries of Galileo, Newton, and other experimental philosophers, but it appears to have been distinctly formulated for the first time by Leibnitz. He at least claims to have first spoken 'of the law of continuity' in a letter to Bayle, printed in the 'Nouvelles de la République des Lettres,' an extract from which is

given in Erdmann's edition of Leibnitz' works, p. 104, under the title 'Sur un Principe Général utile à l'explication des Lois de la Nature[b].' It has indeed been asserted that the doctrine of the *latens processus* of Francis Bacon involves the principle of continuity[c], but I think that this doctrine, like that of the *natures* of substances is merely a vague statement of the principle of causation.

Failure of the Law of Continuity.

There are certain requisite cautions which must be given as to the application of the principle of continuity. In the first place, where this principle really holds true, it may seem to fail owing to our imperfect means of observation. Though a physical law may never admit of perfectly abrupt change, there is no limit to the approach which it may make to abruptness. When we warm a piece of very cold ice, the absorption of heat, the temperature, and the dilatation of the ice vary according to apparently simple laws until we come to the zero of the Centigrade scale. Everything is then changed; an enormous absorption of heat takes place without any rise of temperature, and the volume of the ice decreases as it changes into water. Unless most carefully investigated, this change appears perfectly abrupt; but accurate observation seems to show that there is a certain forewarning; the ice does not turn into water all at once, but through a small fraction of a degree the change is gradual. All the phenomena concerned, if measured very exactly, would be represented not by angular lines, but continuous curves, undergoing rapid flexures; and we may

[b] 'Life of Sir W. Hamilton,' p. 439.

[c] Powell's 'History of Natural Philosophy,' p. 201. 'Novum Organum,' bk. II. Aphorisms 5–7.

probably assert with safety that between whatever points of temperature we examined ice, there would be found some indication, doubtless almost infinitesimally small, of the apparently abrupt change which was to occur at a higher temperature. It might also be pointed out that all the most important and apparently simple physical laws, such as those of Boyle and Marriotte, Dalton and Gay-Lussac, &c., are only approximately true, and the divergences from observation are forewarnings of abrupt changes, which would otherwise break the law of continuity.

Secondly, it must be remembered that mathematical laws of any complexity will probably present singular cases or negative results, which may present the appearance of discontinuity, as when the law of refraction suddenly yields us with perfect abruptness the entirely different phenomenon of total internal reflection. In the undulatory theory there is no real change of law between the phenomenon of refraction and that of reflection.

Faraday in the earlier part of his career found so many substances possessing more or less magnetic power, that he ventured on a great generalization, and asserted that all bodies shared in the magnetic property of iron. His mistake, as he afterwards himself discovered, consisted in overlooking the fact that though magnetic in a certain sense, some substances might have negative magnetism, and be repelled instead of attracted by the magnet. Between magnetism and diamagnetism there must be a zero near or even at which some substances may be classed, but otherwise magnetic properties appear to be universally present in matter.

Thirdly, where we might expect to find a uniform mathematical law prevailing, the law may undergo abrupt change at singular points, and actual discontinuity may arise. We may sometimes be in danger of treating under

one law phenomena which really belong to different laws. It is generally known, for instance, that a spherical shell of uniform matter attracts an external particle of matter with a force varying inversely as the square of the distance from the centre of the sphere. But this law only holds true so long as the particle is external to the shell. Within the shell the law is wholly different, and the aggregate gravity of the sphere becomes zero, because the force in every direction is neutralized by an exactly equal force. If an infinitely small particle be in the superficies of a sphere, the law is again different, and the attractive power of the shell is half what it would be on particles infinitely close to the surface of the shell. Thus in approaching the centre of a shell from a distance, the force of gravity evinces a double discontinuity in passing through the shell[d].

It may well admit of question, too, whether discontinuity is really unknown in nature. We perpetually do meet with events which are real breaks upon the previous law, though the discontinuity may then be a sign that some wholly independent cause has come into operation. If the ordinary course of the tides is interrupted by an enormous and irregular wave, we attribute it to an earthquake, or some gigantic natural disturbance. If a meteoric stone falls upon a person and kills him, it is clearly a discontinuity in his life, of which he could have had no anticipation. A sudden sound may pass through the air neither preceded nor followed by any continuous effect. Although, then, we may regard the Law of Continuity as a principle of nature holding rigorously true in many of the relations of natural forces, it seems to be a matter of difficulty to assign the limits within which the

[d] Thomson and Tait, 'Treatise on Natural Philosophy,' vol. i. pp. 346–351.

law is verified. Much caution, therefore, is desirable in its application.

Negative Arguments on the Principle of Continuity.

Upon the principle of continuity we may often found arguments of great force which prove an hypothesis to be impossible, because it would involve a continual repetition of a process *ad infinitum*, or else a purely arbitrary breach at some point. Bonnet's famous theory of reproduction represented every living creature as containing germs which were perfect representatives of the next generation, so that on the same principle they necessarily included germs of the next generation, and so on indefinitely. The theory was sufficiently refuted when once clearly stated, as in the following poem called the Universe[e], by Henry Baker:

'Each seed includes a plant: that plant, again,
Has other seeds, which other plants contain:
Those other plants have all their seeds, and those
More plants again, successively inclose.

'Thus, ev'ry single berry that we find,
Has, really, in itself whole forests of its kind,
Empire and wealth one acorn may dispense,
By fleets to sail a thousand ages hence.'

The general principle of inference, that what we know of one case must be true of similar cases, if they really are identical in the essential conditions, prevents our asserting anything which we cannot apply time after time under the same circumstances. On this principle Stevinus beautifully demonstrated that weights resting on two inclined planes and balancing each other must be proportional to the lengths of the planes between their apex and a horizontal plane. He imagined an uniform

e 'Philosophical Transactions' (1740), vol. xli. p. 454.

endless chain to be hung over the planes, and to hang below in a symmetrical festoon. If the chain were ever to move by gravity, there would be the same reason for its moving on for ever, and thus producing a perpetual motion. As this is absurd, the portions of the chain lying on the planes, and equal in length to the planes, must balance each other. On similar grounds we may disprove the existence of any *self-moving machine*, for if it could once alter its own state of motion or rest, in however small a degree, there is no reason why it should not do the like time after time *ad infinitum*. Even Newton's proof of his third law of motion, in the case of gravity, is of this character. For he remarks that if two gravitating bodies do not exert exactly equal forces in opposite directions, the one exerting the strongest pull will carry both itself and the other away, and will move with constantly increasing velocity *ad infinitum*. But though the argument might seem sufficiently convincing, Newton in his characteristic way made an experiment with a loadstone and iron floated upon the surface of water[f]. In recent years the very foundation of the principle of conservation of energy has been placed on the assumption that it is impossible by any combination whatever of natural bodies to produce force continually from nothing[g]. The principle admits of frequent application in various subtle forms.

Lucretius attempted to prove, by a most ingenious argument of this kind, that matter must be indestructible. For if a definite quantity, however small, were to fall out of existence in any finite time, an equal quantity might be supposed to lapse in every equal interval of time, so that in the infinity of past time the universe must have ceased to exist[h]. But the argument, however ingenious,

[f] 'Principia,' bk. I. Law iii. Corollary 6.

[g] Helmholtz, Taylor's 'Scientific Memoirs' (1853), vol. vi. p. 118.

[h] 'Lucretius,' bk. I. lines 232–264.

seems to fail at several points. If past time be infinite, why may not matter have been created infinite also? It would be most reasonable, again, to suppose the matter destroyed in any time to be proportional to the matter then remaining, and not to the original quantity; under this hypothesis even a finite quantity of original matter could never wholly disappear from the universe. For like reasons we cannot hold that the doctrine of the Conservation of Energy is really proved, or can ever be proved to be absolutely true, however probable it may be regarded on many grounds.

Tendency to Hasty Generalization.

In spite of all the powers and advantages of generalization, men require no incitement to generalize; they are too apt to draw hasty and ill-considered inferences. As Francis Bacon said, our intellects want not wings, but rather weights of lead to moderate their course[i]. The process is inevitable to the human mind; it begins with childhood and lasts through the second childhood. The child that has once been hurt fears the like result on all similar occasions, and can with difficulty be made to distinguish between case and case. It is caution and discrimination in the adoption of general conclusions that we chiefly have to learn, and the whole experience of life is one continued lesson to this effect. Baden Powell has excellently described this strong natural propensity to hasty inference, and the fondness of the human mind for tracing resemblances real or fanciful. 'Our first inductions,' he says[k], 'are always imperfect and inconclusive; we advance towards real evidence by successive approximations: and accordingly we find false generalization the

i 'Novum Organum,' bk. I. Aphorism 104.

k 'The Unity of Worlds and of Nature,' 2nd edit. p. 16.

besetting error of most first attempts at scientific research. The faculty to generalize accurately and philosophically requires large caution and long training; and is not fully attained, especially in reference to more general views, even by some who may properly claim the title of very accurate scientific observers in a more limited field. It is an intellectual habit which acquires immense and accumulating force from the contemplation of wider analogies.'

Hasty and superficial generalizations have always been the bane of science, and there would be no difficulty in finding endless illustrations. Between things which are the same in number there is a certain resemblance, namely in number, but in the infancy of science men could not be persuaded that there was not a deeper resemblance implied in that of number. Pythagoras was not the inventor of a mystical science of number. In the ancient Oriental religions the seven metals were connected with the seven planets, and in the seven days of the week we still have, and probably always shall have, a relic of the septiform system ascribed by Dio Cassius to the ancient Egyptians. The disciples of Pythagoras carried the doctrine of the number seven into great detail. Seven days are mentioned in Genesis; infants acquire their teeth at the end of seven months; they change them at the end of seven years; seven feet was the limit of man's height; every seventh year was a climacteric or critical year, at which a change of disposition took place. Then again there were the seven sages of Greece, the seven wonders of the world, the seven rites of the Grecian games, the seven gates of Thebes, and the seven generals destined to conquer that city.

In natural science there were not only the seven planets, and the seven metals, but also the seven primitive colours, and the seven tones of music. So deep a

hold did this doctrine take that we still have its results in many customs, not only in the seven days of the week, but the seven years' apprenticeship, puberty at fourteen years, the second climacteric, and legal majority at twenty-one years, the third climacteric. The system was reproduced in the seven sacraments of the Roman Catholic Church, and the seven year periods of Comte's grotesque system of domestic worship. Even in scientific matters the loftiest intellects have occasionally yielded, as when Newton was misled by the analogy between the seven tones of music and the seven colours of his spectrum. Other numerical analogies, though rejected by Galileo, held Kepler in thraldom; no small part of Kepler's labours during seventeen years was spent upon numerical and geometrical analogies of the most baseless character; and he gravely held that there could not be more than six planets, because there were not more than five regular solids. Even the acute genius of Huyghens did not prevent him from inferring that but one satellite could belong to Saturn, because, with those of Jupiter and the Earth, it completed the perfect number of six. A whole series of other superstitions and fallacies attach to the numbers six and nine[1].

It is by false generalization, again, that the laws of nature have been supposed always to possess that simplicity and perfection which we attribute to particular forms and relations. The heavenly bodies, it was held, must move in circles, for the circle was the perfect figure. Even Newton seemed to adopt the questionable axiom that nature must always proceed in the simplest way; in stating his first rule of philosophizing, he adds[m]: 'To this purpose the philosophers say, that nature does nothing in

1 Baring-Gould, 'On the Fatalities of Number,' in 'Curious Myths of the Middle Ages' (1866), p. 209.

m 'Principia,' bk. III. *ad initium.*

vain, when less will serve; for Nature is pleased with simplicity, and affects not the pomp of superfluous causes.' Keill, again, lays down[n] as an axiom that 'The causes of natural things are such, as are the most simple, and are sufficient to explain the phenomena: for nature always proceeds in the simplest and most expeditious method; because by this manner of operating the Divine Wisdom displays itself the more.' If this axiom had any clear grounds of truth, it would not apply to proximate laws; for even when the ultimate law may appear simple the results may be infinitely diverse, as in the various elliptic, hyperbolic, parabolic, or circular orbits of the heavenly bodies. Simplicity is naturally agreeable to a mind of very finite powers, but to an Infinite Mind everything is simple.

Every great advance in science consists in a great generalization, pointing out deep and subtle resemblances. The Copernican system was a generalization, in that it classed the earth among the planets; it was, as Bishop Wilkins expressed it, 'the discovery of a new planet,' but it was opposed by a more shallow generalization. Those who argued from the condition of things upon the earth's surface, thought that every object must be attached to and rest upon something else. Shall the earth, they said, alone be free? Accustomed to certain special results of gravity they could not conceive its action under widely different circumstances[o]. No hasty thinker could seize the deep analogy pointed out by Horrocks between a pendulum and a planet, true in substance though mistaken in some details. All the advances of modern science rise from the conception of Galileo, that in the heavenly bodies, however apparently different their condition, we

[n] Keill, 'Introduction to Natural Philosophy,' p. 89.

[o] Jeremiæ Horroccii 'Opera Posthuma' (1673), pp. 26, 27.

shall ultimately recognise the same fundamental principles of mechanical science which are true on earth.

Generalization is the great prerogative of the intellect, but it is a power only to be exercised safely with much caution and after long training. Every mind must generalize, but there are the widest differences in the depth of the resemblances discovered and the care with which the discovery is verified. There seems to be an innate power of insight which a few men have possessed pre-eminently, and which enabled them, with no exemption indeed from labour or temporary error, to discover the one in the many. Minds of excessive acuteness may exist, which have yet only the powers of minute discrimination, and of storing up, in the treasure-house of memory, vast accumulations of words and incidents. But the power of discovery belongs to a more restricted class of minds. Laplace said that, of all inventors who had contributed the most to the advancement of human knowledge, Newton and Lagrange appeared to possess in the highest degree the happy tact of distinguishing general principles among a multitude of objects enveloping them, and this tact he conceived to be the true characteristic of scientific genius [o].

[o] Young's 'Works,' vol. ii. p. 564.

CHAPTER XXVIII.

ANALOGY.

As we have seen in the previous chapter, generalization passes insensibly into reasoning by analogy, and the difference is but one of degree. We are said to generalize when we view many objects as agreeing in a few properties, so that the resemblance is extensive rather than deep. When we have two or only a few objects of thought, but are able to discover many points of resemblance, we argue by analogy that the correspondence will be even deeper than appears. It may not be true that the words are always used in these distinct senses, and there is no doubt great vagueness in the employment of these and many other logical terms; but, if there is any clear discrimination to be drawn between generalization and analogy, it is indicated above.

It has been often said, indeed, that analogy denotes not a resemblance between things, but between the relations of things. A pilot is a very different man from a Prime Minister, but he bears the same relation to a ship that the minister does to the state, so that we may analogically describe the Prime Minister as the pilot of the state. A man differs still more from a horse, nevertheless four men bear to three men the same relation as four horses bear to three horses; there is the analogy.

Four men : Three men : : Four horses : Three horses, or Four men : Four horses : : Three men : Three horses. There is a real analogy between the tones of the Mono-

chord, the Sages of Thebes, and the Gates of Thebes, but it does not extend beyond the fact that they were all seven in number. Between the most discrete notions, as, for instance, those of time and space, analogy may exist, arising from the fact that the mathematical conditions of the lapse of time and of motion along a line are similar. There is no identity of nature between a word and the thing it signifies; the substance *iron* is a heavy solid, the word *iron* is either a momentary disturbance of the air, or a film of black pigment on white paper; but there is analogy between words and their significates. The substance iron is to the substance iron-carbonate, as the name iron is to the name iron-carbonate, when these names are used according to their correct scientific definitions. The whole structure of language and the whole utility of signs, marks, symbols, pictures, and representations of various kinds, rest upon analogy. I may, perhaps, hope to enter fully upon this important subject at some future time, and to attempt to show how the invention of signs enables us to express, guide, and register our thoughts. It will be sufficient to observe here that the use of words constantly involves analogies of a subtle kind; we should often be at a loss how to describe a notion, were we not at liberty to employ in a metaphorical sense the name of anything sufficiently resembling it. There would be no expression for the sweetness of a melody, or the brilliance of an harangue, unless it were furnished by the taste of honey and the brightness of a torch.

A very cursory examination of the cases in which we popularly use the word analogy, shows that it includes all degrees of resemblance or similarity. The analogy may consist only in similarity of number or ratio; or in like relations of time or space. It may also consist in more simple resemblance between physical properties. We should not be using the word inconsistently with custom,

if we said that there was an analogy between iron, nickel, and cobalt, manifested in the strength of their magnetic powers. There is a still more perfect analogy between iodine and chlorine; not that every property of iodine is identical with the corresponding property of chlorine; for then they would be one and the same kind of substance, and not two substances; but every property of iodine resembles in all but degree some property of chlorine. For almost every substance in which iodine forms a component, a corresponding substance may be discovered containing chlorine, so that we may confidently infer from the compounds of the one to the compounds of the other substance. Potassium iodide crystallizes in cubes; therefore it is to be expected that potassium chloride will also crystallize in cubes. The science of chemistry, as now developed, rests almost entirely upon a careful and most extensive comparison of the properties of substances, bringing to light deep-lying analogies. When any apparently exceptional or new substance is encountered, the chemist is guided in his treatment of it entirely by the analogies which it seems to present with previously known substances.

In this chapter I cannot hope to illustrate the all-pervading influence of analogy in human thought and science. All science, it has been said, at the outset, arises from the discovery of identity, and analogy is but one name by which we denote the deeper-lying cases of resemblance. I shall only try to point out at present how analogy between apparently diverse classes of phenomena often serves as an all-important guide in discovery. We thus commonly gain the first insight into the nature of an apparently unique object, and we thus, in the progress of a science, often discover that we are treating over again, in a new form, phenomena which were well known to us in another form.

Analogy as a Guide in Discovery.

There can be no doubt that discovery is most frequently accomplished by following up hints received from analogy, as Jeremy Bentham remarked[a]. Whenever a phenomenon is perceived, the first impulse of the mind is to connect it with the most nearly similar phenomenon. If we could ever meet a thing wholly *sui generis*, presenting no analogy to anything else, we should be incapable of investigating its nature, except by purely haphazard trial. The probability of success by such a process is so slight, that it is preferable to follow up the slightest clue. As I have pointed out already (vol. ii. p. 24), the possible modifications of condition in experiments are usually infinite in number, and infinitely numerous also are the hypotheses upon which we may proceed. Now it is self-evident that, however slightly superior the probability of success by one course of procedure may be over another, the most probable one should always be adopted first.

The chemist having discovered what he believes to be a new element, will have an infinite variety of modes of treating and investigating it. If in any one of its qualities the substance displays a resemblance to an alkaline metal, for instance, he will naturally proceed to try whether it possesses other properties common to the alkaline metals. Even the apparently simplest phenomenon presents so many points for notice that we have a choice at each moment from among many hypotheses.

It would be difficult to find a more instructive instance of the way in which the mind is guided by analogy than in the description by Sir John Herschel of the course of thought by which he was led to anticipate in theory one of Faraday's greatest experimental discoveries. Sir John

[a] 'Essay on Logic,' 'Works,' vol. viii. p. 276.

Herschel noticed that in three physical phenomena, a screw-like form, technically called helicoidal dissymmetry, was presented, namely in electrical helices, plagihedral quartz crystals, and the rotation of the plane of polarization of light. As he himself has said [b], 'I reasoned thus: Here are three phenomena agreeing in a *very strange peculiarity*. Probably, this peculiarity is a connecting link, physically speaking, among them. Now, in the case of the crystals and the light, this probability has been turned into certainty by my own experiments. Therefore, induction led me to conclude that a similar connexion exists, and must turn up, somehow or other, between the electric current and polarized light, and that the plane of polarization would be deflected by magneto-electricity.' By this course of analogical thought Sir John Herschel had actually been led to anticipate Faraday's great discovery of the influence of magnetic strain upon polarized light. He had tried as long ago as 1822–25 to discover the influence of electricity on light, by sending a ray of polarized light through a helix, or near a long wire conveying an electric current. Such a course of inquiry, followed up with the persistency of Faraday, and with his experimental resources, would doubtless have effected the strange discovery. Herschel also suggests that the plagihedral form of quartz crystals must be due to a screw-like strain during the progress of crystallization; but the notion, although probable, remains unverified by experiment to the present day.

If ever men approach the investigation of the mechanism of thought, they must be guided by analogy. Already many philosophers have drawn analogies between nerve influence and the transmission of vibrations. Dr. Briggs, Newton in his 24th Query, and Hartley, have vaguely speculated concerning such vibrations, and some

[b] 'Life of Faraday,' by Bence Jones, vol. ii. p. 206.

countenance is now given to the notion by the somewhat similar rate of propagation of nerve pulses and sound-waves in soft bodies. But the phenomena of memory are far more difficult to reduce to any material mechanism, and I know of no material analogy but the interesting one suggested by Hooke[c], who likens memory to 'those bells or vases which Vitruvius mentions to be placed in the ancient theatre, which did receive and return the sound more vigorous and strong; or like the unison-toned strings, bells, or glasses, which receive impressions from sounds without, and retain the impressions for some time, answering the tone by the same tone of their own.'

Analogy in the Mathematical Sciences.

Whoever wishes to acquire a deep acquaintance with the constitution of Nature must observe that there are deep analogies which connect whole branches of science in a parallel manner, and enable us to infer of one class of phenomena what we know of the other. It has thus happened on several occasions that the discovery of an unsuspected analogy between two hitherto distinct branches of knowledge has been the starting-point for a rapid course of discovery. The truths readily observed in the one may be of a different character from those which present themselves in the other. The analogy, when once pointed out, leads us easily to discover regions of one science yet undeveloped, but to which the key is furnished by the corresponding truths in the other science. An interchange of aid most wonderful in its results may thus take place, and at the same time the mind rises to a higher generalization, and a more comprehensive view of mind and nature.

[c] 'Posthumous Works,' p. 141.

No two sciences might seem at first sight more entirely discrete and divergent in their subject matter than geometry and arithmetic, or algebra. The first deals with circles, squares, parallelograms, and various other forms in space; the latter with mere symbols of number, the symbols having form indeed, but bearing a meaning independent of shape or size. Prior to the time of Descartes, too, the sciences actually were developed in a slow and painful manner in almost entire independence of each other. The Greek philosophers indeed could not avoid noticing occasional analogies, as when Plato in the Thæetetus describes a square number as *equally equal*, and a number produced by multiplying two unequal factors as *oblong*. Euclid, in the 7th and 8th books of his Elements, continually uses expressions displaying a consciousness of the same analogies, as when he calls a number of two factors a *plane number*, ἐπίπεδος ἀριθμός, and distinguishes a square number of which the two factors are equal as an equal-sided or plane number, ἰσόπλευρος καὶ ἐπίπεδος ἀριθμός. He also calls the root of a cubic number its side, πλευρά. In the Diophantine algebra many problems of a geometrical character were solved by algebraic or numerical processes; but there was no general system, so that the solutions were of an isolated character. In general the ancients were far more advanced in geometric than symbolic methods; thus Euclid in his 4th book gives us the means of dividing a circle by purely geometric or mechanical means into 2, 3, 4, 5, 6, 8, 10, 12, 15, 20, 24, 30 parts, but he was totally unacquainted with the theory of the roots of unity exactly corresponding to this division of the circle.

During the middle ages, on the other hand, algebra advanced beyond geometry, and modes of solving equations were painfully discovered by those who had no notion that at every step they were implicitly solving important

geometric problems. It is true that Regiomontanus, Tartaglia, Bombelli, and possibly other early algebraists, solved isolated geometrical problems by the aid of algebra, but particular numbers were always used, and no consciousness of a general method was displayed. Vieta in some degree anticipated the final discovery, and occasionally represented the roots of an equation geometrically, but it was reserved for Descartes to show, in the most general manner, that every equation may be represented by some curve or figure in space, and that every bend, point, cusp, or other peculiarity in the curve indicates some peculiarity in the values of the algebraic symbols. It is impossible to describe in any adequate manner the importance of this discovery. The advantage was twofold: algebra aided geometry, and geometry gave reciprocal aid to algebra. Curves such as the long described sections of the cone were found to correspond to quadratic equations of no great difficulty; and it was impossible to manipulate the symbolic equations without discovering properties of those all important curves. The way was thus opened for the algebraic treatment of motions and forces, without which Newton's 'Principia' could never have been worked out. Newton indeed was possessed by a strange and, to some extent, unfortunate infatuation in favour of the ancient geometrical methods; but it is well known that he employed symbolic methods to discover his profound truths, and he every now and then, by some accidental use of algebraic expressions, confessed its greater powers and generality.

Geometry, on the other hand, gave the greatest assistance to algebra, by affording concrete representations of relations which would otherwise be too abstract for easy comprehension. A curve of no great complexity may give the whole history of the variations of value of a troublesome mathematical expression. As soon as we know, too, that every regular geometrical curve repre-

sents some algebraic equation, we are presented by simple observation of many mechanical movements with abundant suggestions towards the discovery of mathematical problems. Every particle of a carriage-wheel when moving on a level road is constantly describing a cycloidal curve, the curious properties of which exercised the ingenuity of all the most skilful mathematicians of the seventeenth century, and led to important advancements in algebraic power. It may well be held even that the discovery of the Differential Calculus is mainly due to geometrical analogy, because mathematicians, in attempting to treat algebraically the tangent of a continuously varying curve, were obliged to entertain the notion of infinitely small quantities[d]. There can be no doubt that Newton's fluxional, or in fact geometrical mode of stating the differential calculus, however much it subsequently retarded its progress in England, facilitated its apprehension at first, and I should think it almost certain that Newton discovered the calculus geometrically.

We may accordingly look upon this discovery of analogy, this happy alliance, as Bossut calls it[e], between geometry and algebra, as the chief source of discoveries which have been made for three centuries past in mathematical methods. This is certainly the opinion of no less an authority than Lagrange, who has said, 'So long as algebra and geometry have been separate, their progress was slow, and their employment limited; but since these two sciences have been united, they have lent each other mutual strength, and have marched together with a rapid step towards perfection.'

The advancement of mechanical science has also been greatly aided by analogy. An abstract and intangible

[d] Lacroix, 'Traité Élémentaire de Calcul Différentiel et de Calcul Intégral,' 5 me édit. p. 699.

[e] 'Histoire des Mathématiques,' vol. i. p. 298.

existence like force demands much power of conception, but it has a perfect concrete representative in a line, the end of which may denote the point of application, and the direction the line of action of the force, while the length can be made arbitrarily to denote the amount of the force. Nor does the analogy end here; for the moment of the force about any point, or its product into the perpendicular distance of its line of action from the point, is found to be correctly represented by an area, namely twice the area of the triangle contained between the point and the ends of the line representing the force. Of late years a great generalization has been effected; the Double Algebra of De Morgan is true not only of space relations, but of forces, so that the triangle of forces is reduced to a case of pure geometrical addition. Nay, the triangle of lines, the triangle of velocities, the triangle of forces, the triangle of couples, and perhaps other cognate theorems, are reduced by analogy to one simple theorem, which amounts merely to this, that there are two ways of getting from one angular point of a triangle to another, which ways, though different in length, are identical in their final results[f]. In the wonderful system of quaternions of the late Sir W. R. Hamilton, these analogies are embodied and carried out in the most general manner, so that whatever problem involves the threefold dimensions of space, or relations analogous to those of space, is treated by a symbolic method of the most comprehensive simplicity. Since nearly all physical problems do involve space relations, or those analogous to them, it is difficult to imagine any limits to the work which may be ultimately achieved by this calculus.

It ought to be added that to the discovery of analogy

[f] See Goodwin, 'Cambridge Philosophical Transactions' (1845), vol. viii. p. 269. O'Brien, 'On Symbolical Statics,' Philosophical Magazine, 4th Series, vol. i. pp. 491 &c.

between the forms of mathematical and logical expressions, we undoubtedly owe the greatest recent advance in logical science. Boole based his extension of logical processes entirely upon the notion that logic was an algebra of two quantities, 0 and 1. His profound genius for the investigation of symbolic methods led him to perceive by analogy that there must exist a general system of logical deduction, of which the old logicians had seized only a few stray fragments. Much mistaken as he was in placing algebra as a higher science than logic, no one can deny that the development of the more complex and dependent science had advanced far beyond that of the simpler science, and that Boole, in drawing attention to the connexion, made one of the most important discoveries in the history of science. As Descartes had wedded algebra and geometry, so did Boole substantially accomplish the marriage of logic and algebra.

Analogy in the Theory of Undulations.

There is no class of phenomena which more thoroughly illustrates alike the power and weakness of analogy than the waves which agitate every kind of medium. All waves, whatsoever be the matter through which they pass, obey certain common principles of rhythmical or harmonic motion, and the subject therefore presents a vast field for mathematical generalization. At the same time each kind of medium may allow of waves peculiar in their conditions, so that it is a beautiful exercise in analogical reasoning to observe how, in making inferences from one kind of medium to another, we must make allowance for difference of circumstances. The waves of the ocean are large and visible, and there are the yet greater tidal waves which extend around the globe. From such palpable cases of rhythmical

movement we pass by analogy to waves of sound, varying in length from about 32 feet to a small fraction of an inch. We have but to imagine, if we can, the fortieth octave of the middle C of a piano, and we reach the undulations of yellow light, the ultra-violet being about the forty-first octave. Thus we pass gradually from the palpable and evident to that which is obscure, if not incomprehensible. Yet the very same phenomena of reflection, interference, and refraction, which we find in the one case, may be expected to occur *mutatis mutandis* in the other cases.

From the great to the small, from the evident to the obscure, is not only the natural order in which inference proceeds, but it is the historical order of discovery. The physical science of the Greek philosophers must have remained incomplete, and their theories groundless, because they do not seem ever to have understood the nature and importance of undulations. All their systems were therefore based upon the entirely different notion of continuous movement of translation from place to place. Modern Science tends more and more to the opposite conclusion that all motion is alternating or rhythmical, energy flowing onwards but matter remaining comparatively fixed in position. Diogenes Laertius indeed correctly compared the propagation of sound with the spreading of waves on the surface of water when disturbed by a stone, and Vitruvius displayed a more complete comprehension of the same analogy. It remained for Newton to create the theory of undulatory motion in showing by mathematical deductive reasoning that the particles of an elastic fluid, by vibrating backwards and forwards, might carry forward a pulse or wave moving onwards from the source of disturbance, while the disturbed particles return to their place of rest. He was even able to make a first approximation by theoretical calculation to the velocity of sound-waves in the atmosphere. His theory of sound formed a

hardly less important epoch in science than his far more celebrated theory of gravitation. It opened the way to all the subsequent applications of mechanical principles to the insensible motion of molecules. He seemed to have been frequently, too, upon the brink of another application of the same principles which would have advanced science by at least a century of progress, and made him the undisputed founder of all the theories of matter. He expressed opinions at various times that light might be due to undulatory movements of a medium occupying space, and in one intensely interesting sentence remarks[g] that colours are probably vibrations of different lengths, 'much after the manner that, in the sense of hearing, nature makes use of aërial vibrations of several bignesses to generate sounds of divers tones, for the analogy of nature is to be observed'. He correctly foresaw that red and yellow light would consist of the longer undulations, and blue and violet of the shorter, while white light would be composed of an indiscriminate mixture of waves of various lengths. Newton almost overcame one of the strongest apparent difficulties of the undulatory theory of light, namely, the propagation of light in straight lines. For he observed that though waves of sound bend round an obstacle to some extent, they do not do so in the same degree as water-waves[h]. He had but to extend the analogy proportionally to light-waves, and not only would the difficulty have vanished, but the true theory of diffraction would have been open to him. Unfortunately he had a preconceived theory that rays of light are bent from and not towards the shadow of a body, a theory which for once he did not sufficiently compare with observation to detect its falsity. I am not aware, too, that

g Birch, 'History of the Royal Society,' vol. iii. p. 262, quoted by Young, 'Works,' vol. i. p. 146.

h 'Opticks,' Query 28, 3rd edit. p. 337.

Newton has, in any of his works, displayed an understanding of the phenomena of interference inseparable from the notion of waves.

While the general principles of undulatory or harmonic motion will be the same in whatever medium the motion takes place, the circumstances must often be excessively different. Between light travelling 186,000 miles per second and sound travelling in air only about 1100 feet in the same unit of time, or almost 900,000 times as slowly, we cannot expect a close outward resemblance. There are great differences, too, in the character of the vibrations. Gases scarcely admit of transverse vibration, so that sound travelling in air is a longitudinal wave, the particles of air moving backwards and forwards in the same line in which the wave moves onwards. Light, on the other hand, appears to consist entirely in the movement of points of force transversely to the direction of propagation of the ray. The light-wave is partially analogous to the bending of a rod or of a stretched cord agitated at one end. Now this bending motion may take place in any one of an infinite number of planes, and waves of which the planes are perpendicular to each other cannot interfere any more than two perpendicular forces can interfere. Now the whole of the complicated phenomena of polarized light arise out of this transverse character of the luminous wave, and we must not expect to meet any analogous phenomena in atmospheric sound-waves. It is conceivable that in solids we might produce transverse sound undulations, in which many of the phenomena of polarization might be reproduced. But it would appear that even between transverse sound and light-waves the analogy holds true rather of the principles of harmonic motion than the circumstances of the vibrating medium; from experiment and theory it is inferred that the plane of polarization in plane polarized light is perpendicular to

instead of being coincident with the direction of vibration, as it would be in the case of transverse sound undulations. Thus the laws of elastic forces appear to be essentially different in application to the luminiferous ether and to ordinary solid bodies[1].

Between light and heat, forms of energy, which at first sight appear so different, a perfect analogy has gradually been established. Not only do rays of light and heat obey exactly the same laws of reflection and refraction, but they are subject to exactly the same laws of absorption and polarization. Wherever a light-ray is deficient in the solar spectrum, a heat-ray is also missing. It is now considered that light is but the influence of heat-rays of certain wave-lengths upon the eye, so that we may in fact cease to distinguish radiant heat and rays of light. Heat in the radiant condition is, of course, to be distinguished from the molecular vibration also called heat, and from the potential energy which it produces when absorbed by substances, and rendered latent.

Use of Analogy in Astronomy.

We shall be much assisted in gaining a true appreciation of the value of analogy in its feebler degrees, by considering how much it has contributed to the progress of astronomical science. Our point of observation is so fixed with regard to the universe, and our means of examining distant bodies is so restricted, that we are obliged in many cases to be guided by limited and apparently feeble resemblances. In many cases the result has been confirmed by subsequent direct evidence of the most forcible character.

While the scientific world was divided in opinion

[1] Rankine, 'Philosophical Transactions' (1856), vol. cxlvi. p. 282.

between the Copernican, and Ptolemaic systems, it was analogy which furnished the most satisfactory arguments. Galileo discovered, by the use of his new telescope, the four small satellites which circulate round Jupiter, and make a miniature planetary world. These four Medicean Stars, as they were called, were plainly seen to revolve round Jupiter in various periods, but approximately in one plane, and astronomers irresistibly inferred that what might happen on the smaller scale might also be found true of the greater planetary system. This discovery gave the holding turn, as Sir John Herschel has expressed it, to the opinions of mankind. Even Francis Bacon, who had, in a manner little to the credit of his scientific sagacity, previously opposed the Copernican views, now became partially convinced, saying 'We affirm the solisequium of Venus and Mercury; since it has been found by Galileo that Jupiter also has attendants.' Nor did Huyghens think it superfluous to adopt the analogy as a valid argument[k]. Even in an advanced stage of the science of physical astronomy, the Jovian system has not lost its analogical interest; for the mutual perturbations of the four satellites pass through all their phases within a few centuries, and thus enable us to verify in a miniature case the principles of stability, which Laplace has established for the great planetary system. Oscillations or disturbances which in the motions of the planets appear to be secular, because their periods extend over millions of years, can be watched, in the case of Jupiter's satellites, through complete revolutions within the historical periods of astronomy[l].

In obtaining a knowledge of the stellar universe we must depend much upon somewhat precarious analogies. We must start with the opinion, entertained by Bruno as

k 'Cosmotheoros' (1699), p. 16.

l Laplace, 'System of the World,' vol. ii. p. 316.

long ago as 1591, that the stars may be suns attended perhaps by planets like our earth. This is the most probable first assumption, supported in some degree by very recent spectrum observations, which show the similarity of light derived from many stars with that of the sun. But at the same time we learn by the prism that there are nebulæ and stars in conditions widely different from anything known in our system. In the course of time the analogy may perhaps be restored to comparative completeness by the discovery of many suns in various stages of nebulous condensation. The history of the evolution of our own world may, as it were, be traced back in bodies less developed, or traced forwards in systems more advanced towards the dissipation of energy, and the extinction of life. As in a great workshop, we may perhaps see the material work of Creation as it has variously progressed through thousands of millions of years.

By the careful delineation and classification of the nebulæ and stellar systems, we may hope in time to find some parallel even to that apparently space-filling system of the Milky Way. Michell pointed out that the Pleiades form a remarkable group of worlds, and he thought that it might present an analogy to the sun and its immediate neighbours. The observations of the Herschels and other more recent astronomers, show that we really belong to a vast stratum of worlds of a peculiar split form, including countless myriads of stars of various sizes. The belief in analogy is irresistible, and astronomers have already looked into the depths of space, hoping to find distant nebulous specks which might resemble the supposed form of the Milky Way, and extend our knowledge to a higher order of universes. Such expectations are probably premature, or even unfounded; nevertheless in the forms of the nebulæ we may find much instruction. The spiral form disclosed in many bodies

by Lord Rosse's telescope possesses some analogy to what would happen in a system revolving in a dense retarding medium. Let us once ascertain by the spectroscope that there is a dense envelope of gas, and the forms of those bodies are at once brought into harmony with the laws of matter on this globe. Viewing such worlds as we do from a fixed distant point, they appear variously distorted according to the laws of perspective; but when we find in many objects forms which might have proceeded from the same object variously inclined to the line of vision, analogy will aid us in determining the real form. Thus when we see an apparent nebulous ring, we may be unable to decide whether it is really a ring of matter or a spherical shell, of which the obliquely seen edges are alone apparent. But if elsewhere we discover, as did Lord Rosse, another nebula presenting the distinct appearance of a ring seen edgeways, we may infer with some probability from one case to the other. By similar processes of comparison and analogical reasoning, we may in time assign with much confidence the absolute forms of many classes of celestial objects[m].

In speculations concerning the physical condition of other planets and heavenly bodies, we must often depend upon analogies of a very weak character. We may be said to know that the moon has mountains and valleys, plains and ridges, volcanoes, and streams of lava, and, in spite of the absence of air and water, the rocky surface of the moon presents so many familiar appearances that we do not hesitate to compare them with the features of our own globe. We infer with high probability that Mars has polar snow and an atmosphere absorbing blue rays like our own; Jupiter undoubtedly possesses a cloudy atmosphere, possibly not unlike a magnified copy of that surrounding the earth, but our tendency to adopt an-

[m] Grant's 'History of Physical Astronomy,' pp. 570–571.

alogies receives a salutary correction in the recently discovered fact that the atmosphere of Uranus contains hydrogen. Philosophers of the highest grade have not stopped at these comparatively safe inferences, but have speculated on the existence of living creatures in other planets. Huyghens remarked that as we infer by analogy from the dissected body of a dog to that of a pig and ox or other animal of the same general form, and as we expect to find the same viscera, the heart, stomach, lungs, intestines, &c., in corresponding positions, so when we notice the similarity of the planets in many respects, we must expect to find them alike in other points[n]. He even enters into an inquiry whether the inhabitants of other planets would possess reason and knowledge of the same sort as ours, concluding in the affirmative. Although the power of intellect might be different, he considers that they would have the same geometry if they had any at all, and that what is true with us would be true with them[o]. As regards the sun, he wisely observes that every conjecture fails. Laplace entertained a strong belief in the existence of inhabitants on other planets. The benign influence of the sun gives birth to animals and plants upon the surface of the earth, and analogy induces us to believe that his rays would tend to have a similar effect elsewhere. It is not probable that matter which is here so fruitful of life, would be sterile upon so great a globe as Jupiter, which, like the earth, has its days and nights and years, and changes which indicate active forces. Man indeed is formed for the temperature and atmosphere in which he lives, and, so far as appears, could not live upon the other planets. But there might be an infinity of organizations relative to the diverse constitution of the bodies of the universe. The most active imagination can-

n 'Cosmotheoros' (1699), p. 17.

o Ibid p. 36.

not form any idea of such various creatures, but their existence is not unlikely[p].

We now know that many metals and other elements never found in organic structures are yet capable of forming compounds, with substances of vegetable or animal origin. It is therefore just possible that at different temperatures creatures formed of different but analogous compounds might exist, but it would seem indispensable that carbon should still form the basis of organic structures; for we have no analogies to lead us to suppose that in the absence of that complex element, life can exist. Could we find globes surrounded by atmospheres resembling our own in temperature and composition, we should be almost forced to believe them inhabited, but the probability of any analogical argument decreases rapidly as the condition of a globe diverges from that of our own. The Cardinal Nicholas de Cusa held long ago that the moon was inhabited, but the absence of any appreciable atmosphere renders the existence of inhabitants highly improbable. Speculations resting upon weak analogies hardly belong to the scope of true science, and can only be tolerated as an antidote to the far worse dogmatism which would assert that the thousand million of persons on earth, or rather a small fraction of them, are the sole objects of care of the Power which designed this limitless Universe.

Failures of Analogy.

So constant is the aid which we derive from the use of analogy in all attempts at discovery or explanation, that it is most important to observe in what cases it may lead us into difficulties. That which we expect by analogy to exist may—

[p] 'System of the World,' vol. ii. p. 326. 'Essai Philosophique,' p. 87.

(1) Be found to exist;

(2) May seem not to exist, but nevertheless may really exist;

(3) May actually be non-existent.

In the second case the failure is only apparent, and arises from our obtuseness of perception, the smallness of the phenomenon to be noticed, or the disguised character in which it appears. I have already pointed out that the analogy of sound and light seems to fail because light does not bend round a corner, the fact being that it does so bend in the phenomena of diffraction, which present the effect, however, in such an unexpected and minute form, that even Newton was misled, and turned from the correct hypothesis of undulations which he had partially entertained.

In the third class of cases analogy fails us altogether, and we expect that to exist which really does not exist. Thus we fail to discover the phenomena of polarization in sound travelling through the atmosphere, since air is not capable of any appreciable transverse undulations. These failures of analogy are of peculiar interest, because they make the mind aware of its superior powers. There have been many philosophers who said that we can conceive nothing in the intellect which we have not previously received through the senses. This is true in the sense that we cannot *image* them to the mind in the concrete form of a shape or a colour; but we can speak of them and reason concerning them; in short, we often know them in everything but a sensuous manner. Accurate investigation shows that all material substances retard the motion of bodies through them by substracting energy by impact. By the law of continuity we can frame the notion of a vacuous space in which there is no resistance whatever, nor need we stop there; for we have only to proceed by analogy to the case where a medium should

accelerate the motion of bodies passing through it, somewhat in the mode which Aristotelians attributed falsely to the air. Thus we can frame the notion of *negative density*, and Newton could reason exactly concerning it, although no such thing exists [q].

In every direction of thought we may meet ultimately with similar failures of analogy. A moving point generates a line, a moving line generates a surface, a moving surface generates a solid, but what does a moving solid generate? When we compare a polyhedron, or many-sided solid, with a polygon, or plane figure of many sides, the volume of the first is analogous to the area of the second; the face of the solid answers to the side of the polygon; the edge of the solid to the point of the figure; but the corner, or junction of edges in the polyhedron, is left wholly unrepresented in the plane of the polygon. Even if we attempted to draw the analogies in some other manner, we should still find a geometrical notion embodied in the solid which has no representative in the plain figure [r].

Faraday was able to frame some notion of matter in a fourth condition, which should be to gas what gas is to liquid [s]. Such substance, he thought, would not fall far short of *radiant matter*, by which apparently he meant the supposed caloric or matter assumed to constitute heat, according to the Corpuscular Theory. Even if we could frame the notion, matter in such a state cannot be known to exist, and recent discoveries concerning the continuity of the solid, liquid, and gaseous states remove the basis of the speculation.

From these and many other instances which might be

q 'Principia,' bk. II. Section II. Prop. X.

r De Morgan, 'Cambridge Philosophical Transactions,' vol. xi. Part ii. p. 246.

s 'Life of Faraday,' vol. i. p. 216.

adduced, we learn that analogical reasoning leads us to the conception of many things which, so far as we can ascertain, do not exist. In this way great perplexities have arisen in the use of language and mathematical symbols. All language depends upon analogy; for we join and arrange words so that they may represent the corresponding junctions or arrangements of things and their qualities. But in the use of language we are obviously capable of forming many combinations of words to which no corresponding meaning apparently exists. The same difficulty arises in the use of mathematical signs, and mathematicians have needlessly puzzled themselves about the square root of a negative quantity, which is, in many applications of algebraic calculation, simply a sign without any analogous meaning, there being a failure of analogy.

CHAPTER XXIX.

EXCEPTIONAL PHENOMENA.

If science consists in the detection of identity and the recognition of one uniformity existing in many objects, it follows almost of necessity that the progress of science depends upon the study of exceptional phenomena. Such new phenomena are the raw material upon which we are to exert our faculties of observation and reasoning, in order to reduce the new facts beneath the sway of the laws of nature, either those laws already well known, or those to be discovered. Not only are strange and inexplicable facts those which are on the whole most likely to lead us to some novel and important discovery, but they are also best fitted to arouse our attention. So long as events happen in accordance with our anticipations, and the routine of every-day observation is unvaried, there is nothing to impress upon the mind the smallness of its knowledge, and the depth of mystery, which may be hidden in the commonest sights and objects. In early times the myriads of stars which remained in apparently fixed relative positions upon the heavenly sphere, received far less notice from astronomers than those few planets whose wandering and inexplicable motions formed an unsolved riddle. Hipparchus was induced to prepare the first catalogue of stars, because a single new star had been added to those nightly visible; and in the middle

ages two brilliant but temporary stars caused more popular interest in astronomy than any other events, and to one of them we owe all the observations of Tycho Brahe, the mediæval Hipparchus.

In other sciences, as well as in that of the heavens, exceptional events are commonly the points from which we start to explore new regions of knowledge. It has been beautifully said that Wonder is the daughter of Ignorance, but the mother of Invention; and though the most familiar and slight events, if fully examined, will afford endless food for wonder and for wisdom, yet it is the few peculiar and unlooked-for events which most often lead a scientific mind into a course of discovery. It is true, indeed, that it requires much philosophy to observe things which are too near to us.

The high scientific importance attaching, then, to exceptions, renders it desirable that we should carefully consider the various modes in which an exception may be disposed of; while some new facts will be found to confirm the very laws to which at first sight they seem clearly opposed, others will cause us to limit the generality of our previous statements. In some cases the exception may be proved to be no exception; occasionally it will prove fatal to our previous most confident speculations; and there are some new phenomena which, without really destroying any of our former theories, open to us wholly new fields of scientific investigation. The study of this subject is especially interesting and important, because, as I have before said (vol. ii. p. 233), no important theory can be built up complete and perfect all at once. When unexplained phenomena present themselves as objections to the theory, it will often demand the utmost judgment and sagacity to assign to them their proper place and force. The acceptation or rejection of a theory will entirely depend upon discriminating the one insuperable contra-

dictory fact from many, which, however singular and inexplicable at first sight, may afterwards be shown to be results of wholly different causes, or possibly the most striking results of the very law with which they stand in apparent conflict.

I can enumerate at least eight different classes or kinds of exceptional phenomena, to one or other of which any supposed exception to the known laws of nature will ultimately be referred; they may be briefly described as below, and will be sufficiently illustrated in the succeeding sections.

(1) Imaginary, or false exceptions, that is, facts, objects, or events which are not really what they are supposed to be.

(2) Apparent, but congruent exceptions, which, though apparently in conflict with a law of nature, are really in agreement with it.

(3) Singular exceptions, which really agree with a law of nature, but exhibit remarkable and unique results of it.

(4) Divergent exceptions, which really proceed from the ordinary action of known processes of nature, but which are excessive in amount or monstrous in character.

(5) Accidental exceptions, arising from the interference of some entirely distinct but known law of nature.

(6) Novel and unexplained exceptions, which lead to the discovery of a new series of laws and phenomena, modifying or disguising the effects of previously known laws, without being inconsistent with them.

(7) Limiting exceptions, showing the falsity of a supposed law in cases to which it had been extended, but not affecting its truth in other cases.

(8) Contradictory or real exceptions which lead us to the conclusion that a supposed hypothesis or theory is in opposition to the phenomena of nature, and must therefore be abandoned.

It ought to be clearly understood that in no case is a law of nature really thwarted or prevented from being fulfilled. The effects of a law may be disguised and hidden from our view in some instances—in others the law itself may be rendered inapplicable altogether—but if a law is applicable it must be carried out. Every law of nature must therefore be stated with the utmost generality of all the instances really coming under it. Babbage proposed to distinguish between *universal principles,* which do not admit of a single exception, such as that every number ending in 5 is divisible by five, and *general principles* which are more frequently obeyed than violated, as that 'men will be governed by what they believe to be their interest[a].' But in a scientific point of view general principles must be universal as regards some distinct class of objects, or they are not principles at all. If a law to which exceptions exist is stated without allusion to those exceptions, the statement is erroneous. I have no right to say that 'All liquids expand by heat,' if I know that water below 4° C. does not; I ought to say, 'All liquids, except water below 4° C., expand by heat;' and every new exception discovered will falsify the statement until inserted in it. To speak of some laws as being *generally* true, meaning not universally but in the majority of cases, is a hurtful abuse of the word, but is quite usual. *General* should mean that which is true of a whole *genus* or class, and every true statement must be true of some assigned or assignable class.

Imaginary or False Exceptions.

When a supposed exception to a law of nature is brought to our notice, the first inquiry ought properly

[a] Babbage, 'The Exposition of 1851,' p. 1.

to be—Is there any breach of the law at all? It may be that the supposed exceptional fact is not a fact at all, that it is a mere figment of the imagination. When King Charles requested the Royal Society to investigate the curious fact that a live fish put into a bucket of water does not increase the weight of the bucket and its contents, the Royal Society wisely commenced their deliberations by inquiring whether the fact was so or not. Every statement, however false, must have some cause or prior condition, and the real question for the Royal Society to investigate was, how the King came to think that the fact was so. Mental conditions, as we have seen (vol. ii. p. 4), enter into all acts of observation, and are often a worthy subject of inquiry. But there are many instances in the history of physical science, in which much trouble and temporary error have been caused by false assertions carelessly made, and carelessly accepted without experimental verification.

The reception of the Copernican theory was much impeded by the objection, that if the earth were perpetually moving, a stone dropped from the top of a high tower should be left behind, and should appear to move towards the west, just as a stone dropped from the mast-head of a moving ship would fall behind, owing to the motion of the ship. The Copernicans attempted to meet this grave objection in every way but the true one, namely, that of showing by trial that the asserted facts are not correct ones. In the first place, if a stone had been dropped with suitable precautions from the mast-head of a moving ship, it would have fallen close to the foot of the mast, because by the first law of motion it would remain in the same state of horizontal motion communicated to it by the mast. As the anti-Copernicans had assumed the contrary result as certain to ensue, their argument would of course have fallen through at once. Had the Copernicans next

proceeded to test with great care the other assertion involved, they would have become still better convinced of the truth of their own theory. A stone dropped from the top of a high tower, or into a deep well, would certainly not have been deflected from the vertical direction in the considerable degree required to support the anti-Copernican views; but, with very accurate observation, they might have discovered, as Benzenberg subsequently did, a very small deflection towards the west (vol. i. p. 453). At the moment when a body begins to fall freely, it begins to resemble a very small satellite moving under the force of gravity, as exerted from the earth's centre of attraction, and it therefore describes, like other satellites, a portion of an elliptic orbit[b]. Had the Copernicans then been able to detect and interpret the meaning of this small divergence, they would have found in it a conclusive proof of their own views.

Multitudes of cases might be cited in which laws of nature seem to be evidently broken, but in which the apparent breach entirely arises from a misapprehension of the facts of the case. It is a general law, absolutely true of all crystals yet submitted to examination, that no crystal has a re-entrant angle, that is an angle which towards the axis of the crystal is greater than two right angles. Wherever the faces of a crystal meet they produce a projecting edge, and wherever edges meet they produce a corner. Many crystals, however, when carelessly examined, present exceptions to this law, but closer observation always shows that the apparently re-entrant angle really arises from the oblique union of two distinct crystals. Other crystals seem to possess faces contradicting all the principles of crystallography; but again careful examination shows that the supposed faces are not

[b] 'Cambridge and Dublin Mathematical Journal' (1848), vol. iii. p. 206.

true faces, but surfaces produced by the orderly junction of an immense number of distinct thin crystalline plates, each plate being in fact a separate crystal, in which the laws of crystallography are strictly observed. The roughness of the supposed face, the striæ detected by the microscope, or inference by continuity from other specimens where the true faces of the plates are clearly seen, prove the purely mistaken character of the supposed exception.

In tracing out the isomorphic relations of the elements, great perplexity has often been caused by mistaking one substance for another. It was pointed out that though arsenic was supposed to be isomorphous with phosphorus, the arseniate of soda crystallized in a form distinctly different from that of the corresponding phosphate. Some chemists held this to be a fatal objection to the doctrine of isomorphism; but it was afterwards pointed out by Clarke, that the arseniate and phosphate in question were not corresponding compounds, as they differed in regard to the water of crystallization[c]. Vanadium again appeared to be an exception to the laws of isomorphism, until it was proved by Professor Roscoe, that what Berzelius supposed to be metallic vanadium was really an oxide of vanadium[d].

In the science of crystallography many other apparent exceptions present themselves, and sometimes cause considerable perplexity. Four of the faces of a regular octahedron may become so enlarged in the crystallization of iron pyrites and some other substances, that the other four faces become altogether imperceptible and a regular tetrahedron appears to be produced, contrary to the laws of crystallographic symmetry. Many other crystalline

c Daubeny's 'Atomic Theory,' p. 76.

d 'Bakerian Lecture,' 'Philosophical Transactions,' (1868) vol. clviii. p. 2.

forms are similarly modified, so as to produce a series of what are called *hemihedral* forms.

Apparent but Congruent Exceptions.

Not unfrequently a law of nature will present results in certain circumstances which appear to be entirely in conflict with the law itself. Not only may the action of the law be much complicated and disguised, but it may in various ways be reversed or inverted, so that all careless observers are misled. Ancient philosophers generally believed that while some bodies were heavy by nature, others, such as flame, smoke, bubbles, clouds, &c., were essentially light, or possessed a tendency to move upwards. So acute and learned an inquirer as Aristotle entirely failed to perceive the true nature of buoyancy or apparent lightness, and the doctrine of intrinsic lightness, being expounded in his works, became the accepted view for many centuries. It is true that Lucretius was fully aware why flame tends to rise, holding that—

> 'The flame has weight, though highly rare,
> Nor mounts but when compelled by heavier air.'

Archimedes also was so perfectly acquainted with the buoyancy of bodies immersed in water, that he could not fail to perceive the existence of a parallel effect in air. Yet throughout the early middle ages the light of true science, clear though feeble, could not contend with the powerful but confused glare of the false Peripatetic doctrine. The genius of Galileo and Newton was required to convince people of the simple truth that all matter is heavy, but that the gravity of one substance may be overborne by that of another, as one scale of a balance is carried up by the preponderating weight in the opposite scale. It is curious to find Newton gravely explaining

the difference of absolute and relative gravity, as if it were a new discovery proceeding from his theory[e]. More than a century elapsed before other apparent exceptions to the Newtonian philosophy were explained away.

Newton himself allowed that the motion of the apsides of the moon's orbit appeared irreconcilable with the law of gravity, and it remained for Clairaut to remove the reproach by more complete mathematical analysis. There must always indeed remain, in the motions of the tides or of the heavenly bodies, discrepancies of some amount between theory and observation; but like discrepancies have so often yielded in past times to prolonged investigation that all physicists have come to regard them as merely apparent exceptions, which will afterwards be found to be new confirmations of the law with which they now seem to conflict.

The most beautiful instance, perhaps, which can be adduced of an apparent exception, is found in the total reflection of light, which occurs when a beam of light within a medium falls very obliquely upon the boundary separating it from a rarer medium. It is the general law that when a ray strikes the limit between two media of different refractive indices, part of the light is reflected and part is refracted, but when the obliquity of the ray within the denser medium passes beyond a certain point there is a sudden apparent breach of continuity, and the whole of the light is reflected. A very clear reason can be given for this exceptional conduct of the light; for according to the law of refraction the sine of the angle of incidence always bears a fixed ratio to the sine of the angle of refraction, so that the greater of the two angles, which is always that in the less dense medium, may increase up to a right angle, but when the media differ in refractive power, the less angle cannot become a right angle, as this

[e] 'Principia,' bk. II. Prop. 20. Corollaries, 5 and 6.

would require the sine of an angle to be greater than the radius. It might seem, perhaps, that this was an exception of the kind elsewhere described as a limiting exception, in which a law is shown to be inapplicable beyond certain defined limits; but in the explanation of the exception according to the undulatory theory, we find that there is really no breach or exception to the general law. Whenever an undulation strikes any point in a bounding surface, spherical waves are produced and spread from the point. The refracted ray is the resultant of an infinite number of such spherical waves, and the bending of the ray at the common surface of two media depends upon the comparative velocities of propagation of the undulations in those media. But if a ray falls very obliquely upon the surface of a rarer medium, the waves arising from successive points of the surface may spread so rapidly as never to intersect, and no resultant wave will then be produced. We thus perceive that from general mathematical conditions may arise very distinct apparent effects.

There may occur from time to time distinct failures in our most well-grounded predictions. A comet, of which the orbit has been well determined, may fail, like Lexell's Comet, to appear at the appointed time and place in the heavens. In the present day we should not hold such an exception to our successful predictions to weigh against our belief in the theory of gravitation, but should assume that some unknown body had through the action of gravitation itself deflected the comet. As Clairaut remarked, in publishing his calculations concerning the expected reappearance of Halley's Comet, a body which passes into regions so remote, and which is hidden from our view during such long periods, might be exposed to the influence of forces totally unknown to us, such as the action of other comets, or even of some planet too far removed from the sun to be ever perceived by us. In the case of

Lexell's Comet it was afterwards shown, curiously enough, that its appearance was not one of a regular series of periodical returns within the sphere of our vision, but a single exceptional visit never to be repeated, and probably due to the perturbing powers of Jupiter. Yet this solitary visit was a strong confirmation of the law of gravity with which it seemed to be in conflict.

The division of Biela's Comet into two companion comets was at the time when it occurred one of those unlooked-for and inexplicable events which awaken the attention and interest of observers in the highest degree. Comets indeed have altogether the character of eccentric strangers intruding into our planetary system, and in almost every point they are yet inexplicable; but there is a possibility that the separation of Biela's Comet may prove to be a comparatively ordinary event of cometary history. For if, as is now believed, comets be aggregates of small meteoric stones or particles, forming the denser parts of continuous streams of such bodies circulating round the sun, then it is not unlikely that these aggregates may at times be increased or diminished by the meeting or separation of meteoric streams.

Singular Exceptions.

Among the most interesting of apparent exceptions are those which I propose to call *singular exceptions*, because they are more or less closely analogous to the singular cases, or solutions which occur in mathematical science. A general mathematical law embraces an infinite multitude of cases which have a perfect agreement with each other in a certain respect. It may nevertheless happen that a single case, while obeying the general law, stands out as apparently different from all the rest. The daily rotation of the earth upon its axis gives to all the stars

in the heavens an apparent relative motion of rotation from east to west; but out of countless thousands which obey the rule the Pole Star alone seems to break it. Exact observations indeed show that it also revolves in a small circle, but it might happen for a short time that a star existed so close to the pole that no appreciable change of place would be caused by the daily rotation. It would then constitute a perfect singular exception; for, while really obeying the law, it would break the terms in which it is usually stated. In the same way the poles of every revolving body are singular points.

Whenever the laws of nature are reduced to a mathematical form we may expect to meet with singular cases, and, as all the physical sciences will meet in the mathematical principles of mechanics, there is no part of nature where we may not probably encounter them. In mechanical science itself the circular motion of rotation may be considered a single exception to the rectilineal motion of translation. It is a general law that any number of parallel forces, whether acting in the same or opposite directions, will have a resultant which may be substituted for them with like effect. This resultant will be equal to the algebraic sum of the forces, or the difference of those acting in one direction and the other; it will pass through a point which is determined by a simple formula, and which may be described as the mean point of all the points of application of the parallel forces (vol. i. p. 422). Thus we readily determine the resultant of parallel forces, except in one peculiar case, namely, when two forces are equal and opposite but not in the same straight line. Being equal and opposite the amount of the resultant is nothing, yet, as the forces are not in the same straight line, they do not balance and destroy each other. Examining the formula for the point of application of the resultant, we find that it gives an infinitely great magnitude,

so that the resultant is nothing at all, and acts at an infinite distance, which is practically the same as to say that there is no possible single resultant. Two such forces constitute what is known in mechanical science as a *couple*, which occasions rotatory instead of rectilineal motion, and can only be neutralized by an equal and opposite couple or pair of forces.

The most beautiful instances of singular exceptions are furnished by the science of optics. It is a general law, for instance, that in passing through transparent media the plane of vibration of polarized light remains unchanged. But in certain cases, to which reference has already been frequently made, namely, certain liquids, some peculiar crystals of quartz, and transparent solid media subjected to a magnetic strain, as in Faraday's experiment (vol ii. pp. 234, 287), the plane of polarization is rotated in a screw-like manner. This effect is so entirely *sui generis*, so unlike any other phenomena in nature, as to appear truly exceptional; yet mathematical analysis shows it to be only a single case of much more general laws. As stated by Thomson and Tait[f], it arises from the composition of two uniform circular motions. If while a point is moving round a circle, the centre of that circle move upon another circle, a great variety of curious curves will be produced according as we vary the dimensions of the circles or the rapidity of the motions. In one case where the two circles are exactly equal, the point will be found to move gradually round the centre of the stationary circle, and describe a curious star-like figure connected with the molecular motions out of which the rotational power of the media arises. Among other singular exceptions in optics may be placed the conical refraction of light, already noticed (vol. ii. p. 175), connected with the peculiar form assumed by a wave of light when

[f] 'Treatise on Natural Philosophy,' vol. i. p. 50.

passing through certain double-refracting crystals. The laws obeyed by the wave are exactly the same as in other cases, yet the results are entirely *sui generis*. So far are such cases from contradicting the theory of ordinary cases, that they afford the supreme opportunities for verification.

In astronomy singular exceptions might occur, and in an approximate manner they do occur. We might point to the rings of Saturn as objects which, though undoubtedly obeying the law of gravity, are yet entirely unique, as far as our observation of the universe has gone. They agree, indeed, with the other bodies of the planetary system in the stability of their movements, which never diverge far from the mean position. But a truly singular event might happen, or might have happened, under slightly different circumstances. Had the rings been exactly uniform all round, and with a centre of gravity coinciding for a moment with that of Saturn, a singular case of unstable equilibrium would have arisen, necessarily resulting in the sudden collapse of the rings, and the fall of their debris upon the surface of the planet. Thus in one single case the theory of gravity would give a result wholly unlike anything else known in the mechanism of the heavens.

It is possible that we might meet with singular exceptions in crystallography. If a crystal of the second or dimetric system, in which the third axis is usually unequal to either of the other two, happened to have the three axes equal, it might be mistaken at first sight for a crystal of the cubic system, but would in many ways exhibit different faces and dissimilar properties. There is, again, a possible class of diclinic crystals in which two axes are at right angles and the third axis inclined to the other two. This class is chiefly remarkable for its non-existence in a material point of view, since no crystals

have yet been proved to have such axes. It seems likely that the class would constitute only a singular case of the more general triclinic system, in which all three axes are inclined to each other at various angles. Now if the diclinic form were merely accidental, and not necessitated by any general law of molecular constitution, its actual occurrence would be infinitely improbable, just as it is infinitely improbable that any star should indicate the North Pole with perfect exactness.

In the curves denoting the relation between the temperature and pressure of water there is one very remarkable point entirely single and unique, at which alone water can remain in the three conditions of gas, liquid, and solid in the same vessel. It is the point at which three curves intersect, namely, the steam line showing at what temperatures and pressures water is just upon the point of becoming gaseous, and other similar lines which show when ice is just on the point of melting, and when ice is just about to assume the gaseous state directly.

Divergent Exceptions.

Closely analogous to singular exceptions are those divergent exceptions, in which a phenomenon manifests itself in very unusual magnitude or character, without however in any degree becoming subject to peculiar laws. Thus in throwing ten coins, it happened in four cases out of 2048 throws, that all the coins fell with heads uppermost (vol. i. p. 238); these would usually be regarded as very singular events, and, according to the theory of probabilities, they would be comparatively very rare; yet they proceed only from an unusual conjunction of accidental events, and from no really exceptional causes. In all classes of natural phenomena we may expect to meet with similar divergencies from the average. Sometimes due merely to

the principles of probability, sometimes to deeper reasons. Among every large collection of persons, we shall probably find some persons who are remarkably large or remarkably small, giants or dwarfs, whether in bodily or mental conformation. Such cases appear to be not mere *lusus naturæ*, since they usually occur with a frequency closely accordant with the law of error or divergence from an average, as shown by M. Quetelet and Mr. Galton (vol. i. p. 446). The rise of genius, or the occurrence of extraordinary musical or mathematical faculties, are attributed by M. Galton to the same principle of divergence.

Under this class of exceptions I am inclined to place all kinds of remarkable events arising from an unusual conjunction of many ordinary tendencies. When several distinct forces happen to concur together, we may have surprising or alarming results. Great storms, floods, droughts and other extreme deviations from the average condition of the atmosphere thus arise. They must be expected to happen from time to time, and will yet be very unfrequent compared with minor disturbances. They are not anomalous but only extreme events, exactly analogous to extreme runs of luck. There seems, indeed, to be a fallacious impression in the minds of many persons, that the theory of probabilities necessitates uniformity in the happening of events, so that in the same space of time there will always be closely the same number, for instance, of railway accidents and murders. Buckle has superficially remarked upon the comparative constancy of many such events as ascertained by Quetelet, and some of his readers acquire the false notion that there is a kind of mysterious inexorable law producing uniformity in natural and human affairs. But nothing can be more opposed to the teachings of the theory of probability, which always contemplates the occurrence of extreme and unusual runs of luck. That theory shows

the great improbability that the number of railway accidents per month should be always equal, or nearly so. The public attention is strongly attracted to any unusual conjunction of events, but there is a fallacious tendency to suppose that every such conjunction must be due to a peculiar new cause coming into operation. Unless it can be clearly shown that such unusual conjunctions occur more frequently than they should do according to the theory of probabilities, we should regard them as merely divergent exceptions.

Eclipses and remarkable conjunctions of the heavenly bodies may also be regarded as results of ordinary laws, which nevertheless appear to break the regular course of nature, and never fail to excite surprise or even fear. Such conjunctions of bodies vary greatly in frequency. One or other of the satellites of Jupiter is eclipsed almost every day, but the simultaneous eclipse of three satellites can only take place, according to the calculations of Wargentin, after the lapse of 1,317,900 years. The relations of the four satellites are so remarkable, that it is actually impossible, according to the theory of gravity, that they should all suffer eclipse simultaneously. But it may happen occasionally that while some of the satellites are really eclipsed by entering Jupiter's shadow, the others are either occulted or rendered invisible by passing over his disk, as seen by us. Thus on four occasions, in 1681, 1802, 1826, and 1843, Jupiter has been witnessed in the singular condition of being apparently deprived of satellites. A close conjunction of two planets always excites surprise and admiration, though conjunctions must naturally occur at intervals in the ordinary course of their motions. We cannot wonder, then, that when three or four planets approach each other closely, the event is long remembered. A most exceptional conjunction of Mars, Jupiter, Saturn, and Mercury, which took place in the year 2446 B.C., was adopted

by the Chinese Emperor, Chuen Hio, as a new epoch for the chronology of that Empire, though there is some doubt whether the conjunction was really observed or was calculated from the supposed laws of motion of the planets. It is certain that on the 11th November, 1524, the planets Venus, Jupiter, Mars, and Saturn were seen very close together, while Mercury was only distant by about 16° or thirty apparent diameters of the sun, this conjunction being probably the most remarkable which has occurred in historical times.

Among the perturbations of the planetary motions we may find divergent exceptions arising from the peculiar accumulation or intensification of effects, as in the case of the long inequality of Jupiter and Saturn (vol. ii. p. 70). Leverrier has shown that there is one place between the orbits of Mercury and Venus, and another between those of Mars and Jupiter, in either of which, if a small planet happened to exist, it would suffer comparatively immense disturbance in the elements of its orbit. Now between Mars and Jupiter there do occur the minor planets, the orbits of which are in many cases exceptionally divergent[g].

It is worthy of notice that even in such a subject as formal logic, divergent exceptions seem to occur, not of course due to chance, but exhibiting in an unusual degree a phenomenon which is more or less manifested in all other cases. I pointed out in p. 162 of the first volume, that propositions of the general type $A = BC + bc$ are capable of expression in six equivalent logical forms, so that they manifest in a higher degree than any other proposition yet discovered, the phenomenon of logical equivalency.

Under the head of divergent exceptions we might doubtless place all or nearly all of the instances of substances possessing physical properties in a very high or low degree, which were described in the chapter on

[g] Grant's 'History of Physical Astronomy,' p. 116.

Generalization, (vol. ii. p. 259). Quicksilver is divergent among metals as regards its melting point, and potassium and sodium as regards their specific gravity. Monstrous productions and variations, whether in the animal or vegetable kingdoms, should probably be assigned to this class of exceptions.

Accidental Exceptions.

The third and largest class of exceptions contains those which arise from the casual interference of extraneous causes. A law may be in operation, and, if so, must be perfectly fulfilled, but, while we conceive that we are examining its results, we may have before us the effects of a totally different cause, possessing no connexion with the subject of our inquiry. The law is not really broken, but at the same time the supposed exception is not illusory. It may be a phenomenon which cannot occur but under the condition of the law in question, yet there has been such subsequent interference and modification of the result, that there is an apparent failure of science. There is, for instance, no subject in which more rigorous and invariable laws have been established than in crystallography. As a general rule, each chemical substance possesses its own definite form, by which it can be infallibly recognised; but the mineralogist has to be on his guard against what are called *pseudomorphic* crystals. In some circumstances a substance, having perfectly assumed its proper crystalline form, may afterwards undergo chemical change; a new ingredient may be added, a former one removed, or one element may be substituted for another. In carbonate of lime the carbonic acid is sometimes replaced by sulphuric acid, so that we find gypsum in the form of calcite; other cases are known where the change is inverted and calcite is found in the form of gypsum. Mica, talc, steatite, hematite, are other minerals

subject to these curious transmutations. Sometimes a crystal embedded in a matrix is entirely dissolved away, and subsequently a new kind of mineral is gradually deposited in the cavity as in a mould. Quartz is thus found cast in many forms wholly unnatural to it. A still more perplexing case sometimes occurs. Carbonate of lime is one of the substances capable of assuming two distinct forms of crystallization, in which it bears respectively the names of calcite and arragonite. Now arragonite, while retaining its outward form unchanged, may undergo an internal molecular change into calcite, as indicated by the altered cleavage. Thus we may come across crystals apparently of arragonite, which seem to break all the laws of crystallography, by possessing the cleavage of an entirely different system of crystallization.

Some of the most invariable and certain laws of nature are disguised by interference of unlooked-for causes. While the barometer was yet a new and curious subject of investigation, its theory, as stated by Torricelli and Pascal, seemed to be contradicted by the fact that in a well-constructed instrument the mercury would often stand far above 31 inches in height. Boyle showed[h] that the mercury could be made to rise as much as 75 inches in a perfectly cleansed tube, or about two and a half times as high as could be due to the pressure of the atmosphere. Many absurd theories about the pressure of imaginary fluids were in consequence put forth[i], and the subject was involved in much confusion until the adhesive or cohesive force between glass and mercury, when brought into perfect contact, was pointed out as the real interfering cause.

Guy-Lussac, again, observed that the temperature of boiling water was very different in some kinds of vessels

[h] 'Discourse to the Royal Society,' 28th May, 1684.

[i] Robert Hooke's 'Posthumous Works,' p. 365.

from what it was in others. It is only in contact with metallic surfaces or sharply broken edges that the temperature is at all fixed at 212° Fahr. The suspended freezing of liquids is another case where the action of a law of nature appears to be interrupted. Spheroidal ebullition seemed at first sight a most anomalous phenomenon; it was almost incredible that water should not boil in a red-hot vessel, or that ice could actually be produced in a red-hot crucible. These paradoxical results are now fully explained as due to the interposition of a non-conducting film of vapour between the globule of liquid and the sides of the vessel. The feats of conjurors who handle liquid metals are readily accounted for in the same manner. At one time the *passive state* of steel was regarded as entirely anomalous. It may be assumed as a general law that when two pieces respectively of electro-negative and electro-positive metal are placed in nitric acid, and made to touch each other, the electro-negative metal will undergo rapid solution. But when iron is the electro-negative and platinum the electro-positive, the solution of the iron entirely and abruptly ceases. Faraday ingeniously proved that this effect was due to a thin film of oxide of iron, which forms upon the surface of the iron and protects it[k].

The law of gravity is of so simple and general a character, and is apparently so disconnected from the other laws of nature, that it never suffers any disturbance, and is in no way disguised, but by the complication of its own effects. It is otherwise, however, with those entirely secondary laws of the planetary system, which have only an empirical basis. The fact that all the long known planets and satellites have a similar motion from west to east is not necessitated by any principles of science, but points merely to some common condition existing in the

[k] 'Experimental Researches in Electricity,' vol. ii. pp. 240–245.

nebulous mass from which our system has doubtless been evolved. The retrograde motions of the satellites of Uranus constituted a distinct breach in this law of uniform direction, which became all the more interesting when the single satellite of Neptune was also found to be retrograde. It now became probable, as Baden Powell well observed, that the anomaly would cease to be singular, and become a case of another law, pointing to some general interference, which has taken place on the bounds of the planetary system. Not only have the satellites suffered from this perturbance, but Uranus is also anomalous in having an axis of rotation lying nearly in the ecliptic; and Neptune constitutes a distinct exception to the empirical law of Bode concerning the distances of the planets, which exceptional circumstance may possibly be due to the same disturbance.

Geology is a science in which accidental exceptions are very likely to occur. Only when we find strata in their original relative positions, can we surely infer that the order of succession is the order of time. But it not uncommonly happens that strata are inverted by the bending and doubling action of extreme pressure. Landslips may carry one body of rock into proximity with an unrelated series, and produce results apparently inexplicable[1]. Floods, streams, icebergs, and other casual agents, may occasionally lodge remains in places where they would be wholly unexpected.

Though such interfering causes may have been often wrongly supposed to explain important discoveries, the geologist must of course always bear the possibility of interference in mind. Scarcely more than a century ago it was yet held by many persons that fossils were accidental productions of nature, mere forms into which minerals had been shaped by no peculiar cause. Voltaire

[1] Murchison's 'Silurian System,' vol. ii. p. 733, &c.

appears not to have been able to accept such an explanation; but fearing that the occurrence of fossil fishes on the Alps would support the Mosaic account of the deluge, he did not hesitate to attribute them to the remains of fishes accidentally brought there by travellers or pilgrims. In archæological investigations the greatest caution is requisite in allowing for secondary burials in ancient tombs and tumuli, for imitations, casual coincidences, disturbance by subsequent races, or even by other archæologists, in fact, for a multitude of interfering circumstances. In common life extraordinary events must happen from time to time, as when a shepherdess in France was astonished at an iron chain falling out of the sky near to her feet, the fact being that Guy-Lussac had thrown it out of his balloon, which was passing over her head unseen at the time.

To this class of accidental exceptions I would refer the innumerable breaches of the rules of inflexion in grammar. These rules would be invariable were it not that the forms derived from distinct roots sometimes get mixed together, that mistaken analogies sometimes occasion confusion, and a variety of such disturbing causes produce irregularity. Philology already presents beautiful instances of the manner in which a comprehensive law may be traced out in a thoroughly scientific manner, in spite of apparently inexplicable exceptions.

Novel and Unexplained Exceptions.

When a law of nature appears to fail because some other law has interfered with its action, two cases may obviously present themselves;—the interfering law may be a known and familiar one, or it may have been previously undetected. In the first case, which we have sufficiently considered in the preceding section, we have

nothing to do but calculate as exactly as possible the amount of interference, and make allowance for it; the apparent failure of the law under examination should then disappear. But in the second case the results may be much more important. A phenomenon which entirely fails to be explained by any known laws may indicate the interference of some wholly new series of natural forces. The ancients could not help perceiving that the general tendency of bodies downwards failed in the case of the loadstone, nor would the doctrine of essential lightness explain the exception, since the substance drawn upwards by the loadstone is a heavy metal. We now see clearly that there was no breach in the perfect generality of the law of gravity, but that a new form of energy manifested itself in a conspicuous form in the loadstone for the first time. In this case the forces concerned, those of gravity and electrical attraction, have never yet been brought into correlation with each other.

Other sciences show us that laws of nature, rigorously true and exact, may often be developed by those who are ignorant of far more complex phenomena involved in their application. Newton's comprehension of geometrical optics was sufficient to explain all the ordinary refractions and reflections of light. The simple laws of the bending of rays apply to all rays, whatever the character of the undulations composing them. Newton suspected the existence of other classes of phenomena when he spoke of rays as *having sides;* but it remained for later experimentalists to show that light is a transverse undulation, like the bending of a rod or cord.

Dalton's atomic theory is doubtless true of all chemical compounds, and the essence of it is that the same compound will always be found to contain the same elements in certain definite proportions. Pure calcium carbonate contains 48 parts by weight of oxygen to 40 of calcium,

and 12 of carbon. But when careful analyses were made of a great many minerals, this law often appeared to fail. What was unquestionably the same mineral, judging by its crystalline form and physical properties, would often give varying proportions of its components, and would sometimes contain unusual elements which yet could not be set down as mere impurities. Dolomite, for instance, is a compound of the carbonates of magnesia and lime, but specimens from different places do not exhibit any fixed ratio between the lime and magnesia, and carbonate of iron occasionally forms a real constituent of the mineral. Such facts could be reconciled with the laws of Dalton only by supposing the interference of a new law, that of Isomorphism.

It is now sufficiently established that certain elements are closely related to each other, so that they can, as it were, step into each other's places without apparently altering the form of the compound molecules, or the shape of the crystals which they constitute. The carbonates of iron, calcium, and magnesium, are nearly identical in their crystalline forms, hence they may crystallize together in harmony, producing mixed minerals of considerable complexity, which nevertheless perfectly verify the laws of equivalent proportions. This principle of isomorphism once established, not only explains what was formerly a stumbling-block, but gives most valuable aid to chemists in deciding upon the real constitution of new salts, since those compounds of isomorphous elements which have identical crystalline forms must possess corresponding chemical formulæ.

We may always expect that from time to time new and extraordinary phenomena will be discovered, and will lead to new views of the laws of nature. The recent observation, for instance, that the resistance of a bar of selenium to a current of electricity is affected in an extraordinary

degree by rays of light falling upon the selenium, points to a wholly new relation between light and electricity. The peculiar so-called allotropic changes which sulphur, selenium, and phosphorus undergo by an alteration in the amount of latent heat which they contain, will probably lead at some future time to important inferences concerning the molecular constitution of solids and liquids. The curious substance ozone has perplexed many chemists, and Andrews and Tait thought that it afforded evidence of the decomposition of oxygen by the electric discharge. The researches of Sir B. C. Brodie negative this notion, and afford evidence of the real constitution of the substance [m], which still, however, remains exceptional in its properties and relations, and affords a hope of important discoveries in chemical theory.

Limiting Exceptions.

We may pass to cases where exceptional phenomena are actually irreconcilable with a law of nature previously regarded as true by philosophers. Error must now be allowed to have been committed, but it is obvious that the error may be more or less extensive. It may be that a law holding rigorously true of the facts actually under notice had been extended by generalization to other series of facts then unexamined. Subsequent investigation may show the falsity of this generalization, and the result must be to limit the law for the future to those objects of which it is really true, while we bring the other classes of objects under distinct generalizations. The contradiction to our previous opinions is partial and not total.

Newton laid down as a result of experiment that every ray of homogeneous light has a definite refrangibility, which

[m] 'Philosophical Transactions' (1872), vol. clxii. no. 23.

it preserves throughout its course until extinguished. This is indeed but one case of the general principle of undulatory movement, which Sir John Herschel has stated in the most complete manner under the title, 'Principle of Forced Vibrations' (vol. ii. p. 65), and has asserted to be absolutely universal and without exception. But Sir John Herschel himself described in the 'Philosophical Transactions' for 1845 a curious appearance in a solution of quinine; as viewed by transmitted light the solution appeared colourless, but in certain aspects it possessed a beautiful celestial blue tint. Curiously enough the coloured light comes only from the first portion of liquid which the light enters. Similar phenomena in fluor-spar had been described by Sir D. Brewster in 1838. Professor Stokes, having minutely investigated the phenomena, discovered that they were more or less present in almost all vegetable infusions, and in a number of mineral substances. He came to the conclusion that this phenomenon, called by him Fluorescence, could only be explained by a degradation or alteration in the refrangibility of the rays of light; he asserts, in fact, that light-rays of very short length of vibration in falling upon certain atoms excite undulations of greater length, in total opposition to the principle of forced vibrations. No complete explanation of the mode of change is yet possible, because it evidently depends upon the intimate constitution of the atoms of the substances concerned; but Professor Stokes believes that the principle of forced vibrations is true only so long as the excursions of an atom are very small compared with the magnitude of the complex molecules[n]. It is now also well known that in Calorescence the refrangibility of rays may be increased and the wave-length diminished. Rays of obscure heat and low refrangibility may be concentrated so as to heat a solid substance, and make it give out rays

[n] 'Philosophical Transactions' (1852), vol. cxlii. pp. 465, 548, &c.

belonging to any part of the spectrum, and it seems probable that this effect arises from the impact of distinct but conflicting atoms. Nor is it in light only that we discover limiting exceptions to the law of forced vibrations; for if we closely observe gentle waves lapping upon the stones at the edge of a lake or other piece of water, we shall notice that each larger wave in breaking upon a stone gives rise to a series of waves of a smaller order. Thus there must be constantly in progress a degradation in the magnitude of water-waves. The principle of forced vibrations seems then to be too generally stated by Sir John Herschel, but it must be a very difficult question of mechanical theory to discriminate the circumstances in which it does and does not hold true.

We may sometimes foresee the possible existence of exceptions yet unknown by experience, and limit the statement of our discoveries accordingly. Very extensive inquiries have shown that all substances yet examined fall into one of two classes; they are all either ferro-magnetic, that is, magnetic in the same way as iron, or they are diamagnetic like bismuth. But it does not thence follow that every substance must be ferro-magnetic or diamagnetic. The magnetic properties are shown by Sir W. Thomson[o] to depend upon the specific inductive capacities of the substance in three rectangular directions. If these inductive capacities are all positive, we have a ferro-magnetic substance; if negative, a diamagnetic substance; but if the specific inductive capacity were positive in one direction and negative in the others, we should have an exception to previous experience, and could not place the substance under either of the present recognised classes.

So many gases have been reduced to the liquid state, and so many solids fused, that scientific men rather hastily

[o] 'Philosophical Magazine,' 4th Series, vol. i. p. 182.

adopted the generalization that all substances could exist in all three states. A certain number of gases, such as oxygen, hydrogen, and nitrogen, have resisted all efforts to liquefy them, and it now seems probable from the experiments of Dr. Andrews that they are limiting exceptions. Dr. Andrews finds that above 88° Fahr. carbonic acid cannot be liquefied by any pressure he could apply, whereas below this temperature liquefaction is always possible. By analogy it becomes highly probable that even hydrogen might be liquefied if cooled to a sufficiently low temperature. We must modify our previous views, and either assert that *below a certain critical temperature* every gas may be liquefied, or else we must assume that a highly condensed gas is, when above the critical temperature, undistinguishable from a liquid. At the same time we receive an explanation of a remarkable exception presented by liquid carbonic acid to the general rule that gases expand more by heat than liquids. This liquid carbonic acid was found by Thilorier in 1835 to expand more than four times as much as air; but by the light of Dr. Andrews' experiments we may learn to regard the liquid as rather a highly condensed gas than an ordinary liquid, and it is actually possible to reduce the gas to the apparently liquid condition without any abrupt condensation[p].

It is an empirical law of the planetary system that all the bodies composing it revolve from west to east; that law is broken, as we have seen, in the cases of one planet and several satellites, probably by the interference of an accidental disturbing force. The law also fails to be true of comets, which, taken as a whole, appear to move according to no single uniform law. This exception, however, is one of limitation only, for in all probability comets, although at present members of our system, have not

[p] Maxwell, 'Theory of Heat,' p. 123.

always been so, but have, in wandering through space, been entangled in our system and retained by the attractive influence of Jupiter, or one of the other larger planets. We must then limit the statement of the law of uniform direction to bodies which are derived from the original constituents of the nebulous mass.

Limiting exceptions occur most frequently in the natural sciences of Botany, Zoology, Geology, &c., the laws of which are almost wholly empirical. In innumerable instances the confident belief of one generation has been falsified by the wider observation of a succeeding one. Aristotle confidently held that all swans are white[q], and the proposition seemed true until not a hundred years ago black swans were discovered in Western Australia. At one time all the animal remains discovered in the Scottish Old Red Sandstone were fishes or shells, until at last a single small air-breathing reptile occurred opportunely to prevent any hasty conclusions[r]. In zoology and physiology we may expect a fundamental identity to exist in the vital processes, but continual discoveries show that there is no limit to the apparently anomalous expedients by which life is reproduced. Alternate generation, fertilization for several successive generations, hermaphroditism, are opposed to all we should expect from induction founded upon the higher animals. But such phenomena are only limiting exceptions showing that what is true of one class is not true of another. In certain of the cephalopoda we meet the extraordinary fact that an arm of the male is cast off and lives independently until it encounters the female.

[q] 'Prior Analytics,' ii. 2, 8, and elsewhere.

[r] Murchison's 'Siluria' (1854), p. 254.

Real Exceptions to Supposed Laws.

The exceptions which we have lastly to consider, are perhaps the most important of all, since they lead to the entire rejection of a law or theory before accepted. No law of nature can fail; there are no such things as real exceptions. Where contradiction exists it must be in the mind of the experimentalist. Either the law is imaginary or the phenomena which conflict with it; if, then, by our senses we can satisfy ourselves of the actual occurrence of the phenomena, the law must be rejected as illusory. The followers of Aristotle held that nature abhorred a vacuum, and thus accounted for the rise of water in a pump. When Torricelli pointed out the visible fact that water would not rise more than 33 feet in a pump, nor mercury more than about 30 inches in a glass tube, they attempted to represent these facts as limiting exceptions, saying that nature abhorred a vacuum to a certain extent and no further. But the Academicians del Cimento completed their discomfiture by showing that if we remove the pressure of the surrounding air, and in proportion as we remove it, nature's feelings of abhorrence decrease and finally disappear altogether. Even Aristotelian doctrines could not stand such direct contradiction.

Lavoisier's ideas concerning the constitution of acids received complete refutation. He named oxygen the *acid generator*, because he believed that all acids were compounds of oxygen, a generalization based on insufficient data. Berthollet, as early as 1789, proved by analysis that hydrogen sulphide and prussic acid, both clearly acting the part of acids, were devoid of oxygen; the former might perhaps have been interpreted as a limiting exception, but when so powerful an acid as hydrogen chloride

(muriatic acid) was found to contain no oxygen the theory had to be relinquished. Berzelius' theory of the dual formation of chemical compounds has met a similar fate.

It is obvious that all conclusive *experimenta crucis* constitute real exceptions to the supposed laws of the theory which is overthrown. Newton's corpuscular theory of light was not rejected on account of its absurdity or inconceivability, for in these respects it is, as we have seen, far superior to the undulatory theory. It was rejected because certain small diffraction fringes of colour did not appear in the exact place and of the exact size which calculation showed that they ought to appear according to the conditions of the theory (vol. ii. pp. 145–151). One single fact clearly irreconcilable with a theory involves its total rejection. In the greater number of cases, what appears to be a fatal exception, may be afterwards explained away as a singular or disguised result of the very laws with which it seems to conflict, or as due to the interference of extraneous causes; but if we fail thus to reduce the fact to congruity, it remains more powerful than any theories or any dogmas.

Of late years not a few of the favourite doctrines of geologists have been rudely destroyed. It was the general belief that human remains were to be found only in those deposits which are actually in progress at the present day, so that the creation of man appeared to have taken place at the beginning, as it were, of this geological age. The discovery of a single worked flint in older strata and in connexion with the remains of extinct mammals was sufficient to explode such a doctrine. Similarly, the opinions of geologists have been altered by the discovery of the Eozoön in the Laurentian rocks of Canada; it was previously held that no remains of life occurred in any older strata than those of the Silurian system. As the exami-

nation of the strata of the globe becomes more and more complete, our views of the origin and succession of life upon the globe must undergo many changes and extensions.

Unclassed Exceptions.

At every period of scientific progress there will necessarily exist a multitude of exceptional and unexplained phenomena which we know not how to regard. They are the outstanding facts upon which the labours of investigators must be exerted,—the ore from which the gold of future discovery is to be extracted. It might be thought that, as our knowledge of the laws of nature increases, the number of such exceptions should decrease; but, on the contrary, the more we know the more there is yet to learn and explain. This arises from several reasons; in the first place the principal laws and forces in nature are numerous, so that he who bears in mind the wonderfully large numbers developed in the doctrine of combinations, will anticipate the existence of almost infinitely numerous relations of one law to another. When we are once in possession of a law, we are potentially in possession of all its consequences; but it does not follow that the mind of man, so limited in its powers and capacities, can actually work them all out in detail. Just as the aberration of light was discovered empirically, though it should have been foreseen, so there are doubtless multitudes of unexplained facts, the connexion of which with laws of nature already known to us, we should perceive, were we not hindered by the imperfection of our deductive powers. But, in the second place, as will be more fully pointed out, it is not to be supposed that we have in any degree approximated to an exhaustion of nature's powers. The

most familiar facts may teem with indications of forces, now secrets hidden from us, because we have not mind-directed eyes to discriminate them. The progress of science will consist in the discovery from time to time of new exceptional phenomena, and their assignment by degrees to one or other of the heads already described. When a new fact proves to be merely a false, apparent, singular, divergent, or accidental exception, we may gain a more minute and accurate acquaintance with the effects of certain laws already known to exist. We have indeed no addition to what was implicitly in our possession, but, as already explained, there is much difference between knowing the laws of nature and perceiving all their complicated effects. Should a new fact prove to be a limiting or real exception, we have to alter, in part or in whole, our views of nature and are saved from errors into which we had fallen. Lastly, the new fact may come under the sixth class, and may eventually prove to be a novel and unexplained phenomenon, indicating the existence of new laws and forces, complicating but not otherwise interfering with the effects of laws and forces previously known.

The best instance which I can find of an unresolved exceptional phenomenon, consists in the anomalous vapour-densities of phosphorus, arsenic, mercury, and cadmium. It is one of the most important laws of chemistry, discovered by Gay-Lussac, that equal volumes of gases exactly correspond to equivalent weights of the substances, and this holds generally true of any elements which we can convert into gas or vapour. Unfortunately phosphorus and arsenic give vapours exactly twice as dense as they should do by analogy, and mercury and cadmium diverge in the other direction, giving vapours half as dense as we should expect. We cannot treat these anomalies as limiting exceptions, and say that the law holds true of sub-

stances generally but not of these; for the properties of gases, as previously noticed (vol. ii. p. 250), usually admit of the surest and widest generalizations. Besides, the preciseness of the ratio of divergence points to the real observance of the law in a modified manner. We might endeavour to reduce the exceptions by doubling the atomic weights of phosphorus and arsenic, and halving those of mercury and cadmium. But this step has of course been maturely considered by chemists, and is found to conflict with all the other analogies of the substances and the principles of isomorphism. One of the most probable explanations is that phosphorus and arsenic produce vapour in an allotropic condition, which might perhaps by intense heat be resolved into a simple gas of half the density; but facts are wholly wanting to support this hypothesis, and it cannot be applied to the other two exceptions without supposing that gases and vapours generally are capable of resolution into something simpler. In short, chemists can at present make nothing of these anomalies. As Hofmann distinctly says, 'Their philosophical interpretation belongs to the future . . . They may turn out to be typical facts, round which many others of the like kind may come hereafter to be grouped; and they may prove to be allied with special properties, or dependent on particular conditions as yet unsuspected[8].'

The expansion of solids and liquids by heat is also a general law, in which we cannot expect to find any real anomalies, any facts indicating too wide generalization, or even any accidental disturbing causes. The contraction of water and several other liquids, even of fusible metal, by heat, together with the few cases in which a solid contracts by heat, must therefore be probably regarded as results of the very law of expansion acting in a

[8] Hofmann's, 'Introduction to Chemistry,' p. 198.

complicated and disguised manner. It would be easy to point out an almost infinite number of other unexplained anomalies. Physicists assert, as an absolutely universal law, that in liquefaction heat is absorbed[t], yet sulphur is at least an apparent exception.

The two substances, Sulphur and Selenium, are remarkable for their relations to heat. Sulphur may almost be said to have two melting points, for, though liquid like water at 120° C., it becomes quite thick and tenacious between 221° and 249°, melting once again at higher temperatures. As well as the other element named, it may be thrown into several curious states, which chemists conveniently dispose of by calling them *allotropic*, a term freely used when they are puzzled to know what has happened. The chemical and physical history of iron, again, is full of anomalies; not only does it undergo inexplicable changes of hardness and texture in its alloys with carbon and other substances, but it is almost the only substance which conveys sound with greater velocity at a higher than at a lower temperature, the velocity increasing from 20° to 100° C., and then decreasing. Silver is also anomalous in regard to sound. These are all instances of inexplicable exceptions, the bearing of which must be ascertained in the future progress of science.

When the discovery of new and peculiar phenomena conflicting with our theories of the constitution of nature is reported to us, it becomes no easy task to steer a philosophically correct course between credulity and scepticism. We are not to assume, on the one hand, that there is any limit to the wonders which nature can present to us. Nothing except the contradictory is really impossible, and many things which we now regard as common-place were

[t] Stewart's 'Elementary Treatise on Heat,' p. 80.

considered as little short of the miraculous when first perceived. The electric telegraph was a visionary dream among mediæval physicists; it has hardly yet ceased to excite our wonder; to our descendants centuries hence it will probably appear inferior in ingenuity to some inventions which they will possess. Now every strange phenomenon may be a secret spring which, if rightly touched, will open the door to new chambers in the palace of nature. To refuse to believe, then, in the occurrence of anything new and strange would be to neglect the most precious chances of discovery. We may say with Hooke that 'the believing strange things possible may perhaps be an occasion of taking notice of such things as another would pass by without regard as useless.' We are not, therefore, to shut our ears even to such apparently absurd stories as those concerning second sight, clairvoyance, animal magnetism, ode force, table-turning, or any of the popular delusions which from time to time are current. The facts recorded concerning these matters are facts in some sense or other, and they demand explanation, either as new natural phenomena, or as the results of combined credulity and imposture. Most of the statements concerning the supposed phenomena referred to have been, or by careful investigation would doubtless be, referred to the latter head, and the absence of any appearance of scientific ability or care in many of those who describe them, is sufficient to cast a doubt upon their value. It is mainly upon this ground, and not on account merely of the strangeness and intrinsic improbability of the statements made that we should hesitate to accept them. Certainly in the obscure phenomena of mind, those relating to memory, dreams, somnambulism, and other peculiar actions or states of the nervous system, there are many inexplicable and almost incredible facts, and it is equally unphilosophical to believe or to disbelieve without clear

evidence. There are many facts, too, concerning the instincts of animals, and the mode in which they find their way from place to place, which are at present quite inexplicable. We may always feel sure that there are many things not yet dreamt of in our philosophy.

CHAPTER XXX.

CLASSIFICATION.

THE extensive subject of Classification has been deferred to a late part of this treatise, because it involves many questions of difficulty, and did not seem naturally to fall into any earlier place. But it must not be supposed that, in now formally taking up the subject, we are for the first time entertaining the notion of classification. All logical inference involves classification, which is indeed the necessary accompaniment of the action of judgment. It is impossible to detect a point of similarity between two or more objects without thereby joining them together in thought, and thus forming an incipient or potential class. Nor can we ever bestow a common name upon two or more objects without thereby equally implying the existence of a class. Every common name is the name of a class, and every name of a class is a common name. It is evident also that every general notion, or concept is but another way of speaking of a class. Usage alone leads us to use the word classification in some cases and not in others. We are said to form the general notion *parallelogram* when we regard an infinite number of possible four-sided rectilinear figures as resembling each other in the common property of possessing parallel sides. We should be said to form a class, Trilobite, when we place alongside of each other in a museum a number of hand specimens resembling each other in certain defined qualities. But

the logical nature of the operation is, or should be, exactly the same in both cases. We form a class of figures called parallelograms, and we form a general notion of Trilobites.

Science, it has been said at the outset, is the detection of identity, and classification is the placing together, either in thought or in actual proximity of space, those notions or objects between which identity has been detected. Accordingly the value of classification is co-extensive with the value of science and general reasoning. Whenever we form a class we reduce multiplicity to unity, and detect, as Plato said, the one in the many. The result of such classification is to yield generalized knowledge, as distinguished from the direct and sensuous knowledge of particular facts. Of every class, so far as it is correctly formed, the great principle of substitution is true, and whatever we know of one object in a class we also know of the other objects, so far as identity has been detected between them. The facilitation and abbreviation of mental labour is at the bottom of all mental progress. The reasoning faculties of Newton were not different in qualitative character from those of a ploughman; the difference lay in the extent to which they were exerted, and the number of facts which could be treated. Every thinking being generalizes more or less, but it is the depth and extent of his generalizations which distinguish the philosopher. Now it is the exertion of the classifying and generalizing powers which thus enables the intellect of man to cope in some degree with the infinite number and variety of natural phenomena and objects. In the chapters upon Combinations and Permutations it was rendered quite evident, that from a few elementary differences immense numbers of various combinations can be produced. The process of classification enables us to resolve these combinations, and refer each one to its place according to

one or other of the elementary circumstances out of which it was produced. We restore nature, as it were, to the simple conditions out of which its endless variety was developed. As Professor Bowen has excellently said[a], 'The first necessity which is imposed upon us by the constitution of the mind itself, is to break up the infinite wealth of Nature into groups and classes of things, with reference to their resemblances and affinities, and thus to enlarge the grasp of our mental faculties, even at the expense of sacrificing the minuteness of information which can be acquired only by studying objects in detail. The first efforts in the pursuit of knowledge, then, must be directed to the business of Classification. Perhaps it will be found in the sequel, that Classification is not only the beginning, but the culmination and the end, of human knowledge.'

Classification Involving Induction.

The purpose of classification must always be the detection of resemblances and laws of nature. However much the process may in some cases be disguised, classification is not really distinct from the process of perfect induction, whereby we endeavour to ascertain the connexions which exist between the several properties of the objects under treatment. There can be no use in placing an object in a class unless something more than the fact of being in that class is thereby implied. If we arbitrarily formed a class of metals and placed therein a selection from the list of known metals made by the ballot—we should have no reason to expect that the metals in question would resemble each other in any points except that they are

[a] 'A Treatise on Logic, or, the Laws of Pure Thought,' by Francis Bowen, Professor of Moral Philosophy in Harvard College, Cambridge, United States, 1866, p. 315.

metals, and have been selected by the ballot. But when chemists carefully selected from the list the five metals, Potassium, Sodium, Cæsium, Rubidium, and Lithium, and called them the Alkaline metals, a great deal was implied in this classification. On comparing the qualities of these substances, they are all found to combine very energetically with oxygen, to decompose water at all temperatures, and to form strongly basic oxides, which are very soluble in water, yielding powerfully caustic and alkaline hydrates from which water cannot be expelled by heat. Their carbonates are also soluble in water, and each metal forms only one chloride. It may also be expected as a general rule that each salt into which one of the five metals enters will correspond to salts into which the other metals enter, there being a general analogy between the properties and compounds of these metals.

Now in forming this class of alkaline metals, we have done more than merely select a convenient order of statement. We have arrived at a discovery of certain empirical laws of nature, the probability being very considerable that a metal which exhibits some of these properties will also possess the others. If we discovered another metal whose carbonate was soluble in water, and which energetically combined with water at all temperatures, producing a strongly basic oxide, we should infer that it would form only a single chloride, and that, generally speaking, it would enter into a series of compounds corresponding to the salts of the other alkaline metals. The formation of this class of alkaline metals, then, is no mere matter of convenience; it is an important and highly successful act of inductive discovery, enabling us to register many undoubted propositions as results of perfect induction, and to make an almost indefinite series of inferences depending upon the principles of imperfect induction.

Professor Huxley has defined the process of classification in the following terms[b]. 'By the classification of any series of objects, is meant the actual or ideal arrangement together of those which are like and the separation of those which are unlike; the purpose of this arrangement being to facilitate the operations of the mind in clearly conceiving and retaining in the memory the characters of the objects in question.'

This statement is doubtless correct, so far as it goes, but it does not include all that Professor Huxley himself implicitly treats under classification. He is fully aware that deep correlations, or in other terms deep uniformities or laws of nature, will be disclosed by any well chosen and profound system of classification. I should therefore propose to modify the above statement, as follows:—'By the classification of any series of objects, is meant the actual or ideal arrangement together of those which are like and the separation of those which are unlike, the purpose of this arrangement being, primarily, to disclose the correlations or laws of union of properties or circumstances, and, secondarily, to facilitate the operations of the mind in clearly conceiving and retaining in the memory the characters of the objects in question.'

Multiplicity of Modes of Classification.

In approaching the question how any given group of objects may best be classified, let it be remarked that there must generally be an unlimited number of modes of classifying any group of objects. Misled, as we shall see, by the problem of classification in the natural sciences, philosophers often seem to think that in each subject there must be one essentially natural classification which

[b] 'Lectures on the Elements of Comparative Anatomy,' 1864, p. 1.

is to be selected, to the exclusion of all others. This erroneous notion probably proceeds also in part from the limited powers of thought and the inconvenient mechanical conditions under which we labour. If we arrange the books in a library catalogue, we must arrange them in some one order; if we compose a treatise on mineralogy, the minerals must be successively described in some one arrangement; if we describe even such simple things as geometrical figures, they must be taken in some fixed order. We shall naturally therefore select that classification which appears to be most convenient and instructive for our principal purpose. But it does not follow that this system of classification possesses any exclusive excellence, and there will be usually many other possible arrangements, each valuable in its own way. A perfect intellect would not confine itself to one order of thought, but would simultaneously regard a group of objects as classified in all the ways of which they are capable. Thus the elements may be classified according to their atomicity into the groups of Monads, Dyads, Triads, Tetrads, Pentads, and Hexads, and this is probably the most instructive classification; but it does not prevent us from also classifying them according as they are metallic or non-metallic, solid, liquid or gaseous at ordinary temperatures, useful or useless, abundant or scarce, ferro-magnetic or diamagnetic, and so on.

Mineralogists have spent a great deal of labour in trying to discover a so-called natural system of classification for minerals. They have constantly encountered the difficulty that the chemical composition did not run together with the crystallographic form, and the various physical properties of the mineral. Substances identical in the form of their crystals, especially those belonging to the first or cubical system of crystals, were often found to have no resemblance in chemical compo-

sition. The identically same substance, again, is occasionally found crystallized in two essentially different crystallographic forms; calcium carbonate, for instance, appearing as calc-spar and arragonite. Now the simple truth is that if we are unable to discover any correspondence, or, as we shall call it, any *correlation* between the several properties of a mineral, we cannot make any one arrangement which will enable us to treat at any one time all these properties. We must really classify minerals in as many different methods as there are different unrelated properties of sufficient importance. Even if, for the purpose of describing minerals successively in some one order in a treatise, we select one system, that, for instance, having regard to chemical composition, we ought mentally at least to regard the same minerals as classified in all other possible modes.

Exactly the same may be said of the classification of plants. An immense number of different modes of classifying plants have been proposed at one time or other, an exhaustive account of which will be found in Rees' 'Cyclopædia,' article 'Classification,' or in the Introduction to Lindley's 'Vegetable Kingdom.' There have been the Fructistæ, such as Cæsalpinus, Morison, Hermann, Boerhaave or Gaertner, who arranged plants according to the form of the fruit. The Corollistæ, Rivinus, Ludwig, and Tournefort, paid attention chiefly to the number or arrangement of the parts of the corolla. Magnol selected the calyx as the critical part, while Sauvage arranged plants according to their leaves; nor are these instances more than a small selection from the actual variety of modes of classification which have been tried. Of such attempts it may be said that every proposed system will probably yield some information concerning the relations of plants, and it is only after trying many modes that it is possible to approximate to the best.

Natural and Artificial Systems of Classification.

It has been usual to distinguish systems of classification as natural and artificial, those being called natural which seemed to express the order of existing things as determined by nature. Artificial methods of classification, on the other hand, included those formed for the mere convenience of men in remembering or treating natural objects.

The difference, as it is commonly regarded, has been well described by Ampère[c], as follows: 'We can distinguish two kinds of classifications, the natural and the artificial. In the latter kind, some characters, arbitrarily chosen, serve to determine the place of each object; we abstract all other characters, and the objects are thus found to be brought near to or to be separated from each other, often in the most bizarre manner. In natural systems of classification, on the contrary, we employ concurrently all the characters essential to the objects with which we are occupied, discussing the importance of each of them; and the results of this labour are not adopted unless the objects which present the closest analogy are brought most near together, and the groups of the several orders which are formed from them are also approximated in proportion as they offer more similar characters. In this way it arises that there is always a kind of connexion, more or less marked, between each group and the group which follows it.'

There is much, however, that is vague and logically false in this and many other definitions which have been proposed by naturalists to express their notion of a natural system. We are not informed how the *import-*

[c] 'Essai sur la Philosophie des Sciences', p. 9.

ance of a resemblance is to be determined, nor what is the measure of the *closeness* of analogy. Until all the words employed in a definition are made clear in meaning, the definition itself is worse than useless. Now if the views concerning classification here upheld are true, there can be no sharp and precise distinction between natural and artificial systems. All arrangements which serve any purpose at all must be more or less natural, because, if closely enough scrutinized, they will involve more resemblances than those whereby the class was defined.

It is true that in the biological sciences there would be one arrangement of plants or animals which would be conspicuously instructive, and in a certain sense natural, if it could be attained, and it is that after which naturalists have been in reality striving for nearly two centuries, namely, that arrangement which would display the genealogical descent of every form from the original life germ. Those morphological resemblances upon which the classification of living beings is almost always based are inherited resemblances, and it is evident that descendants will usually resemble their parents and each other in a great many points.

I have said that a natural is distinguished from an arbitrary or artificial system only in degree. It will be found almost impossible to arrange objects according to any one circumstance without finding that some correlation of other circumstances is thus made apparent. No arrangement could seem more arbitrary than the common alphabetical arrangement according to the initial letters of the name. But we cannot scrutinize a list of names of persons without noticing a predominance of Evans's and Jones's, under the letters E and J, and of names beginning with Mac under the letter M. The predominance is so great that we could not attribute it to chance, and inquiry would of course show that it arose from im-

portant facts concerning the nationality of the persons. It would appear that the Evans's and Jones's were of Welsh descent, and those whose names bear the prefix Mac of Scotch descent. With the nationality would be more or less strictly correlated many peculiarities of physical constitution, language, habits, or mental character. In other cases I have been interested in noticing the empirical inferences which are displayed in the most apparently arbitrary arrangements. If a large register of the names of ships be examined it will often be found that a number of ships bearing the same name were built about the same time, a correlation due to the occurrence of some striking incident shortly previous to the building of the ships. The age of ships or other structures is usually closely correlated with their general form, nature of materials, &c.

It is impossible to examine the details of some of the most apparently artificial systems of classification of plants, without finding that many of the classes are natural in character. Thus in Tournefort's arrangement, depending almost entirely on the formation of the corolla, we find the natural orders of the Labiatæ, Cruciferæ, Rosaceæ, Umbelliferæ, Liliaceæ, and Papilionaceæ, recognised in his 4th, 5th, 6th, 7th, 9th, and 10th classes. Many of the classes in Linnæus' celebrated sexual system also approximate to natural classes.

Correlation of Properties.

Habits and usages of language are always apt to lead us into the error of imagining that when we employ different words we mean different things. In introducing the subject of classification nominally I was careful to draw the reader's attention to the fact that all reasoning and all operations of scientific method really involve classification, though we are accustomed to use the name

in some cases and not in others. Now the name *correlation* requires to be used with the same qualification. Things are correlated (*con, relata*) when they are so related or bound to each other that where one is the other is, and where one is not the other is not. Throughout this work we have then been dealing with correlations. In geometry the occurrence of three equal angles in a triangle is correlated with the existence of three equal sides; in physics gravity is correlated with inertia; in botany exogenous growth is correlated with the possession of two cotyledons, or the production of flowers with that of spiral vessels. But it is in the classificatory sciences especially that the word correlation has been employed.

We find it stated that in the class Mammalia the possession of two occipital condyles, with a well-ossified basi-occipital, is correlated with the possession of mandibles, each ramus of which is composed of a single piece of bone, articulated with the squamosal element of the skull, and also with the possession of mammæ and non-nucleated red blood-corpuscles. Professor Huxley remarks[d] that this statement of the character of the class mammalia is something more than an arbitrary definition; it is a statement of a law of correlation or co-existence of animal structures, from which most important conclusions are deducible. It involves a generalization to the effect that in nature the structures mentioned are always found associated together. This simply amounts to saying that the formation of the class mammalia involves an act of inductive discovery, and results in the establishment of certain empirical laws of nature. Professor Huxley has excellently expressed the mode in which discoveries of this kind enable naturalists to make deductions or predictions

[d] 'Lectures on the Elements of Comparative Anatomy, and on the Classification of Animals,' 1864, p. 3.

with considerable confidence, but he has also pointed out that such inferences are likely from time to time to prove mistaken. I will quote his own words:

'If a fragmentary fossil be discovered, consisting of no more than a ramus of a mandible, and that part of the skull with which it articulated, a knowledge of this law may enable the palæontologist to affirm, with great confidence, that the animal of which it formed a part suckled its young, and had non-nucleated red blood-corpuscles; and to predict that should the back part of that skull be discovered, it will exhibit two occipital condyles and a well-ossified basi-occipital bone.

'Deductions of this kind, such as that made by Cuvier in the famous case of the fossil opossum of Montmartre, have often been verified, and are well calculated to impress the vulgar imagination; so that they have taken rank as the triumphs of the anatomist. But it should carefully be borne in mind, that, like all merely empirical laws, which rest upon a comparatively narrow observational basis, the reasoning from them may at any time break down. If Cuvier, for example, had had to do with a fossil Thylacinus instead of a fossil Opossum, he would not have found the marsupial bones, though the inflected angle of the jaw would have been obvious enough. And so, though, practically, any one who met with a characteristically mammalian jaw would be justified in expecting to find the characteristically mammalian occiput associated with it; yet, he would be a bold man indeed, who should strictly assert the belief which is implied in this expectation, viz., that at no period of the world's history did animals exist which combined a mammalian occiput with a reptilian jaw, or *vice versâ*.'

One of the most distinct and remarkable instances of correlation in the animal world is that which occurs in ruminating animals, and which could not be better stated

than in the following extract from the classical work of Cuvier[e]:

'I doubt if any one would have divined, if untaught by observation, that all ruminants have the foot cleft, and that they alone have it. I doubt if any one would have divined that there are frontal horns only in this class: that those among them which have sharp canines for the most part lack horns.

'However, since these relations are constant, they must have some sufficient cause; but since we are ignorant of it, we must make good the defect of the theory by means of observation: it enables us to establish empirical laws which become almost as certain as rational laws when they rest on sufficiently repeated observations; so that now whoso sees merely the print of a cleft foot may conclude that the animal which left this impression ruminated, and this conclusion is as certain as any other in physics or morals. This footprint alone, then, yields to him who observes it, the form of the teeth, the form of the jaws, the form of the vertebræ, the form of all the bones of the legs, of the thighs, of the shoulders, and of the pelvis of the animal which has passed by: it is a surer mark than all those of Zadig.'

We meet with a good instance of the purely empirical correlation of circumstances when we classify the planets of the solar system according to their densities or periods of axial rotation[f]. If we examine a table specifying the usual astronomical numbers of the solar system, we find that four planets resemble each other very closely in the period of axial rotation, and the same four planets are all found to have high densities, thus:—

[e] 'Ossemens Fossiles,' 4th edit. vol. i. p. 164. Quoted by Huxley, 'Lectures,' &c., p. 5.

[f] Chambers, 'Descriptive Astronomy,' 1st edit. p. 23.

Name of Planet.	Period of Axial Rotation.	Density.
Mercury	24 hours 5 minutes	7·94
Venus	23 „ 21 „	5·33
Earth	23 „ 56 „	5·67
Mars	24 „ 37 „	5·84

Forming a similar table for the other chief planets, it is as follows :—

Name of Planet.	Period of Axial Rotation.	Density.
Jupiter	9 hours 55 minutes	1·36
Saturn	10 „ 29 „	·74
Uranus	9 „ 30 „	·97
Neptune	— —	1·02

It will of course be observed that in neither group is the equality of the rotational period or of the density more than rudely approximate, nevertheless the difference of the numbers in the first and second group is so very marked, the periods of the first being at least double and the densities four or five times those of the second, that the coincidence cannot be attributed to accident. The reader will also notice that the first group consists of the planets nearest to the sun, that with the exception of the earth none of them possess satellites, and that they are all comparatively small; the second group are furthest from the sun, and all of them possess several satellites, and are comparatively great. Therefore, with but slight exception, the following correlations hold true :—

Interior planets.	Long period.	Small size.	High density.	No satellites.
Exterior „	Short „	Great „	Low „	Many „

These coincidences certainly point with much probability to a difference in the conditions of origin of the two groups, but no further explanation of the matter is yet possible.

The classification of comets by Mr. Hind and Mr. A. S. Davis according to their periods tends to establish the conclusion that distinct groups of comets have been

brought into the solar system by the attractive powers of Jupiter, Uranus, or other planets[g]. The classification of nebulæ as commenced by the two Herschels, and continued by Lord Rosse, Mr. Huggins, and others, will probably lead at some future time to the discovery of important empirical laws concerning the constitution of the universe. The minute examination and classification of meteorites, as carried on by Mr. Sorby and others, seems likely to afford us an insight into the constitution of the material universe.

We should never fail to remember and record the slightest and most apparently inexplicable coincidences or correlations, for they may prove of importance in the future. Discoveries begin when we are least expecting them. It is a very significant fact that the greater number of variable stars are of a reddish colour. Not all variable stars are red, nor all red stars variable, but considering that only a small fraction of the observed stars are known to be variable, and only a small fraction are red, the number which fall into both classes is too great to be accidental[h]. It is also remarkable that the greater number of stars possessing great proper motion are double stars, the star 61 Cygni being especially noticeable in this respect[i]. The correlation in these cases is not perfect and without exception, but the preponderance is so great as to point to some natural correlation, the exact nature of which must be a matter for future investigation. Sir John Herschel has remarked that the two double stars 61 Cygni and α Centauri of which the orbits were well ascertained, evidently belonged to the same family or genus[k].

g 'Philosophical Magazine,' 4th Series, vol. xxxix. p. 396; vol. xl. p. 183; vol. xli. p. 44. h Humboldt, 'Cosmos,' (Bohn) vol. iii. p. 224.

i Baily, 'British Association Catalogue,' p. 48.

k 'Outlines of Astronomy,' § 850, 4th edit. p. 578.

Classification in Crystallography.

One of the most perfect and instructive instances of classification which we can find is furnished by the science of crystallography, already briefly noticed (vol. i. p. 153). The system of arrangement now generally adopted is conspicuously natural, and is even mathematically perfect. A crystal consists in every part of similar molecules similarly related to the adjoining molecules, and connected with them by forces the nature of which we can only learn by their apparent effects. But these forces are exerted in space of three dimensions, so that there is a limited number of suppositions which can be entertained as to the relations of these forces. In one case each molecule will be similarly related to all those which are next to it; in a second case, it will be similarly related to those in a certain plane, but differently related to those not in that plane. In the simpler cases the arrangement of molecules is rectangular; in the remaining cases oblique either in one or two planes.

In order to simplify the explanation and conception of the complicated phenomena which crystals exhibit, an hypothesis has been invented which is an excellent illustration of the class of Descriptive Hypotheses before mentioned (vol. ii. p. 153). Crystallographers imagine that there are within each crystal certain axes, or lines of direction, by the comparative length and the mutual inclination of which the nature of the crystal is determined and recorded. In one somewhat exceptional class of crystals there are three such axes lying in one plane, and a fourth perpendicular to that plane; but in all the other classes there are imagined to be only three axes. Now these axes can be varied in three ways as regards

length: (1) they may be all equal, or (2) two equal and one unequal, or (3) all unequal. They may also be varied in four ways as regards direction: (1) they may be all at right angles to each other; (2) two axes may be at right angles and the third perpendicular to one of them and oblique to the other; (3) two axes may be at right angles to each other and the third oblique to both; (4) the three axes may be all oblique to each other. Now if all the variations as regards length were combined with those regarding direction, it would seem to be possible to have twelve classes of crystals in all, the enumeration being then logically and geometrically complete. But as a matter of empirical observation, many of these classes are not found to occur, oblique axes being seldom or never equal. There remain in all seven distinct classes of crystals, but even of these one class is not positively known to be represented in nature.

The first class of crystals is defined by possessing three equal rectangular axes, and equal elasticity in all directions. The primary or most simple form of the crystals is the cube, but by the modification or removal of the corners of the cube by planes variously inclined to the axes, we have the regular octohedron, the dodecahedron, or various combinations of these forms. Now it is a law of this class of crystals that as each axis is exactly like each of the other two, every modification of any corner of a crystal must be repeated symmetrically with regard to the other axes; thus the forms produced are symmetrical or regular, and the class is called the *Regular System* of Crystals. It includes a great variety of substances, some of them being elements, such as carbon in the form of diamond, others more or less complex compounds, such as rock-salt, potassium iodide and bromide, the several kinds of alum, fluor-spar, iron bisulphide, garnet, spinelle, &c. No correlation then is apparent between the form of

crystallization and the chemical composition. But what we have to notice is that the physical properties of the crystallized substances with regard to light, heat, electricity, &c., are closely similar. Light and heat undulations, wherever they enter a crystal of the regular system, spread with equal rapidity in all directions, just as they would in a uniform liquid, gas, or amorphous solid, such as unstrained glass. Crystals of the regular system accordingly do not in any case exhibit the phenomena of double refraction, unless by mechanical compression we alter the conditions of elasticity. These crystals, again, expand equally in all directions when heated, and if we could out a sufficiently large plate from a cubical crystal, and examine the sound vibrations of which it is capable, we should find that they indicated an equal elasticity in every direction. Thus we see that a great number of important properties are correlated with that of crystallizing in the regular system, and as soon as we know that the primary form of crystallization of a substance is the cube, we are able to infer with approximate certainty that it possesses all these properties. The class of cubical crystals is then an evidently natural class, one disclosing general laws connecting together the physical and mechanical properties of the substances so classified.

In the second class of crystals, called the dimetric, square prismatic, or pyramidal system, there are also three axes at right angles to each other, two of which are equal, and the third or principal axis is unequal, being either greater or less than either of the other two. In such crystals accordingly the elasticity and other physical properties are alike in all directions perpendicular to the principal axis, but vary in all other directions. If a point within a crystal of this system be heated, the heat spreads with equal rapidity in planes perpendicular to the prin-

cipal axis, but more or less rapidly in the direction of this axis, so that the isothermal surface is an ellipsoid of revolution round that axis.

Nearly the same statement may be made concerning the third or hexagonal or rhombohedral system of crystals, in which there are three axes lying in one plane and meeting at angles of 60°, while the fourth axis is perpendicular to the other three. The hexagonal prism and the rhombohedron are the two commonest forms assumed by crystals of this system, and in ice, quartz, and calc-spar, we have abundance of beautiful specimens of the various forms produced by the modification of the primitive form. Calc-spar alone is said to crystallize in at least 700 varieties of forms. Now of all the crystals belonging both to this and the dimetric class, we know that a ray of light passing in the direction of the principal axis will be refracted singly as in a crystal of the regular system; but in every other direction the light will suffer double refraction being separated into two rays, one of which obeys the ordinary law of refraction, but the other a much more complicated law. The other physical properties vary in an analogous manner. Thus calc-spar expands by heat in the direction of the principal axis, but contracts by a small quantity in directions perpendicular to it. So closely indeed are these various physical properties correlated that Mitscherlich, having observed the law of expansion in calc-spar, was enabled to predict that the double refracting power of the substance would be decreased by a rise of temperature, as was proved by experiment to be the case.

In the fourth system, called the trimetric, rhombic, or right prismatic system, there are three axes, at right angles, but all unequal in length. It may be asserted in general terms that the mechanical properties vary in such crystals in every direction, and heat spreads so that

the isothermal surface is an ellipsoid with three unequal axes.

In the remaining three classes, called the monoclinic, diclinic, and triclinic, the axes are more or less oblique, as described above (vol. ii. p. 360), and at the same time unequal. The complication of phenomena is therefore greatly increased, and it need only be stated that there are always two directions in which a ray is singly refracted, but that in all other directions double refraction takes place. The conduction of heat is unequal in all directions, the isothermal surface being an ellipsoid of three unequal axes. The relations of such crystals to other phenomena are often very complicated, and hardly yet reduced to law. Thus some crystals, called pyro-electric, manifest vitreous electricity at some points of their surface, and resinous electricity at other points when rising in temperature, the character of the electricity being changed when the temperature sinks again. This production of electricity is believed indeed to be connected with the hemihedral character of the crystals exhibiting it. The crystalline structure of a substance again influences its magnetic behaviour, the general law being that the direction in which the molecules of a crystal are most closely approximated tends to place itself axially or equatorially between the poles of a magnet, according as the body is magnetic or diamagnetic. Further questions arise if we apply pressure to crystals. Thus doubly refracting crystals with one principal axis acquire two axes when the pressure is perpendicular in direction to the principal axis.

All the phenomena peculiar to crystalline bodies are thus closely correlated with the formation of the crystal, or will almost certainly be found to be so as investigation proceeds. It is upon empirical observation indeed that the laws of connexion are in the first place founded, but

the simple hypothesis that the elasticity and approximation of the particles vary in the directions of the crystalline axes allows of the application of deductive reasoning. The whole of the phenomena are gradually being proved to be consistent with this hypothesis, so that we have in this subject of crystallography a beautiful instance of successful classification, connected with a nearly perfect physical hypothesis. Moreover this hypothesis was verified experimentally as regards the mechanical vibrations of sound by Savart, who found that the vibrations in a plate of biaxial crystal indicated the existence of varying elasticity in varying directions.

Classification an Inverse and Tentative Operation.

If all attempts at so-called natural classification be really attempts at perfect induction, it follows that they are all subject to the remarks which were made upon the inverse character of the inductive process, and upon the difficulty of every inverse operation (vol. i. pp. 14, 15, 140, &c.). There will of necessity be no royal road to the discovery of the best system, and it will even be impossible to lay down any series of rules of procedure to assist those who are in search of a good arrangement. The only invariable logical rule which could be stated would be as follows :—Having given certain objects, group them in every way in which they can be grouped, and then observe in which method of grouping the coincidence of properties is most conspicuously manifested. But this method of exhaustive classification will in almost every case be impracticable, owing to the immensely great number of modes in which a comparatively small number of objects may be grouped together. About sixty-three elements have been classified by chemists in six principal groups as Monad, Dyad, Triad, &c. elements, the numbers

in the classes varying from three to twenty elements. Now if we were to calculate the whole number of ways in which sixty-three objects can be arranged in six groups, we should find the number to be so great that the life of the longest lived man would be wholly inadequate to enable him to go through these possible groupings. The rule of exhaustive arrangement, then, is absolutely impracticable. It follows also that mere haphazard trial cannot as a general rule give any useful result. If we were to write the names of the elements in succession upon sixty-three cards, throw them into a ballot-box, and draw them out haphazard in six handfuls time after time, the probability is excessively small that we take them out at any one trial in a specified order, for instance that at present adopted by chemists.

The usual mode in which an investigator proceeds to form a classification of any new group of objects, seems to consist in tentatively arranging them according to their most obvious similarities. Any two objects which present a close resemblance to each other will be joined and formed into the rudiment of a class, the definition of which will at first include all the apparent points of resemblance. Other objects as they come to our notice will be gradually assigned to those groups with which they present the greatest number of points of resemblance, and the definition of a class will often have to be altered in order to admit them. The early chemists, for instance, could hardly avoid classing together the common metals, gold, silver, copper, lead, and iron, which present such conspicuous points of similarity as regards density, metallic lustre, malleability, &c. With the progress of discovery, however, difficulties begin to present themselves in such a grouping. Antimony, bismuth, and arsenic are distinctly metallic as regards lustre, density, and some chemical properties, but are wanting in malle-

ability. The more recently discovered and rare tellurium presents greater difficulties, for it has many of the physical properties of metal, and yet all its chemical properties are analogous to those of sulphur and selenium which have never been regarded as metals. Great chemical differences again are by degrees discovered between the five metals just mentioned; and the class, if it is to have any chemical validity, must be made to include other elements, having none of the original properties on which the class was founded. Hydrogen is a transparent colourless gas and the least dense of all substances, yet in its chemical analogies it is a metal, as suggested by Faraday[m] in 1838, and almost proved by the late Professor Graham; it must be placed in the same class as silver. In this way it comes to pass that almost every classification which is proposed in the early stages of a science will be found to break down as the deeper similarities of the objects come to be detected. The most obvious points of difference will have to be neglected. Chlorine is a gas, bromine a liquid, and iodine a solid, and at first sight these might have seemed formidable circumstances to overlook; but in chemical analogy the substances are closely united. The progress of organic chemistry, too, has yielded wholly new ideas of the similarities of compounds. Who, for instance, would recognise without extensive research a close similarity between glycerine and alcohol, or between fatty substances and ether. The class of paraffins contains three substances gaseous at ordinary temperatures, several liquids, and some crystalline solids. It required much insight to detect the perfect affinity which exists between such apparently different substances.

The science of chemistry now depends to a great extent on a correct classification of the elements, as will be learnt by consulting the able article on Classification by Pro-

[m] 'Life of Faraday,' vol. ii. p. 87.

fessor G. C. Foster in Watts's 'Dictionary of Chemistry.' But the present theory of classification was not reached until at least three previous false systems had been long entertained. And though there is much reason to believe that the present system of classification according to atomicity is substantially correct, many errors may yet be discovered in the details of the grouping.

Symbolic Statement of the Theory of Classification.

The whole theory of classification can be explained in the most complete and general manner, by reverting for a time to the use of the Logical Abecedarium, which was found to be of supreme importance in Formal Logic (vol. i. p. 109). That form expresses in fact the necessary classification of all objects and ideas as depending on the laws of thought, and there is no point concerning the purpose and methods of classification which may not be explained most precisely by the use of letter combinations, the only inconvenience being the somewhat abstract and repulsive form in which the subject is thus represented.

If we pay regard only to three qualities or circumstances in which things may resemble each other, namely the qualities A, B, C, then there are according to the laws of thought eight possible classes of objects. If there exist objects belonging to all these eight classes, thus indicated,

ABC	*a*BC
AB*c*	*a*B*c*
A*b*C	*ab*C
A*bc*	*abc*

it follows that the qualities A, B, C are subject to no conditions except the primary laws of thought and nature (vol. i. p. 6). There is then no special law of nature to

discover, and, if we arrange the classes in any one order rather than another, it must be for the purpose of showing that the combinations are logically complete. It will be obvious that there are three different possible arrangements which may be of some use; firstly, that employed above in which all the combinations containing A stand first, and those devoid of it follow; secondly, and thirdly, the similar arrangements in which the combinations containing B, and C, respectively stand first.

Suppose now that there are but four kinds of objects possessing the qualities A, B, C, and that these kinds are represented by the combinations ABC, A*b*C, *a*B*c*, *abc*. The order of arrangement will now be of importance; for if we place them in the order

$$\left\{\begin{matrix}\mathrm{ABC}\\ a\mathrm{B}c\end{matrix}\right.\quad\left\{\begin{matrix}\mathrm{A}b\mathrm{C}\\ abc\end{matrix}\right.$$

placing the B's first and those which are *b*'s last, we shall perhaps overlook the law of correlation of properties involved. But if we arrange the combinations as follows

$$\left\{\begin{matrix}\mathrm{ABC}\\ \mathrm{A}b\mathrm{C}\end{matrix}\right.\quad\left\{\begin{matrix}a\mathrm{B}c\\ abc\end{matrix}\right.$$

it becomes apparent at once that where A is, and only where A is, the property C is to be found, B being indifferently present and absent. The second arrangement then would be called a natural one, as rendering manifest the conditions under which the combinations exist.

As a further instance, let us suppose that eight objects are presented to us for classification, which exhibit combinations of the five properties, A, B, C, D, E, in the following manner:—

ABC*d*E	*a*BC*d*E
AB*cde*	*a*B*cde*
A*b*CDE	*ab*CDE
A*bc*D*e*	*abc*D*e*.

They are now classified, so that those containing A stand first, and those devoid of A second, but no other property seems to be correlated with A. Let us alter this arrangement and group the combinations as follows:—

ABC*d*E	A*b*CDE
AB*cde*	A*bc*D*e*
*a*BC*d*E	*ab*CDE
*a*B*cde*	*abc*D*e*.

It requires very little examination to discover that, in the first group, B is always present and D absent, whereas in the second group, B is always absent and D present. This is the result which follows from a law of the form $B = d$ (see vol. i. p. 157), so that in this mode of arrangement we readily discover a close correlation between two letters. Altering the groups again as follows:—

ABC*d*E	AB*cde*
*a*BC*d*E	*a*B*cde*
A*b*CDE	A*bc*D*e*
*ab*CDE	*abc*D*e*,

we discover another evident correlation between C and E. Between A and the other letters, or between the two pairs of letters B, D and C, E there is no logical connexion whatever.

This example may perhaps seem tedious, but it will be found instructive in this way. We are classifying only seven objects or combinations, in each of which only five qualities are considered. There are only two laws of correlation between four of those five qualities, and those laws are of the simplest logical character. Yet the reader would hardly discover what those laws were, and confi-

dently assign them by mere contemplation of the combinations, as given in the first group. Several tentative classifications must probably be made before we can resolve the question. Let us now suppose that instead of seven objects and five qualities, we have, say, five hundred objects and fifty qualities. If we were to attempt the same method of exhaustive grouping which we before employed, we should have to arrange the five hundred objects in fifty different ways, before we could be sure that we had discovered even the simpler laws of correlation. But even the successive grouping of all those possessing each of the fifty properties would not necessarily give us all the laws. There might exist complicated relations between several properties simultaneously, for the detection of which no rule of procedure whatever can be given.

If the reader entertains any doubt as to the difficulty of classifying combinations so as to disclose their relations, let him test the matter practically upon the following series of combinations. They involve only six letters denoting six qualities, which are subject to four laws of correlation of no great complexity.

ABCDEF	ABc*D*e*f*
ABCD*e*F	A*bcd*E*f*
ABCD*ef*	*a*Bc*D*EF
ABC*d*E*f*	*a*Bc*D*eF
ABc*D*EF	*a*Bc*D*e*f*
ABc*D*eF	*abcd*E*f*.

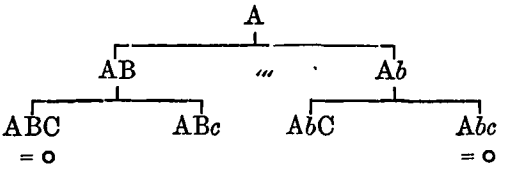
I shall be happy to receive the solution of the above problem in classification from any reader who thinks he has solved it; that is to say, I shall be glad to ascertain whether any reader succeeds in detecting the laws of correlation between the letters, which yield the above combinations, according to the principles of the Indirect Method described in Chapter VI.

Bifurcate Classification.

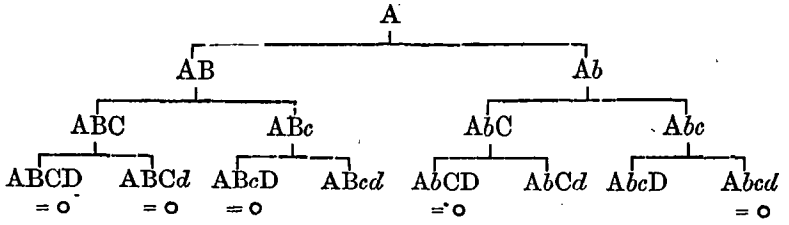
Every system of classification ought theoretically to be formed on the principles of the Logical Abecedarium. Each superior class should be divided into two inferior classes, distinguished by the possession and non-possession of a single specified property. Each of these minor classes, again, is divisible by any other property whatever which can be suggested, and thus every classification logically consists of an infinitely extended series of subaltern genera and species. The classifications which we form are in reality very small fragments of those which would correctly and fully represent the relations of existing things. But if we take more than four or five qualities into account, the number of subdivisions grows impracticably large. Our finite minds are unable to treat any complex group exhaustively, and we are obliged to simplify and generalize scientific problems, often at the risk of overlooking particular conditions and exceptions.

Every system of classes displayed in the manner of the Logical Abecedarium may be called *bifurcate*, because every class branches out at each step into two minor classes, existent or imaginary. It would be a great mistake to regard this arrangement as in any way a peculiar or special method; it is not only a natural and important one, but it is the inevitable and only system which is logically perfect, according to the fundamental laws of thought. All other arrangements of classes correspond to the bifurcate arrangement, with the implication that some of the minor classes are not represented among existing things. If we take the genus A and divide it into the species AB and AC, we imply two propositions, namely that in the class A, the properties of B and C

never occur together, and that they are never both absent; these propositions are indeed logically equivalent to one, namely $AB = Ac$. Our classification is then identical with the following bifurcate one :—



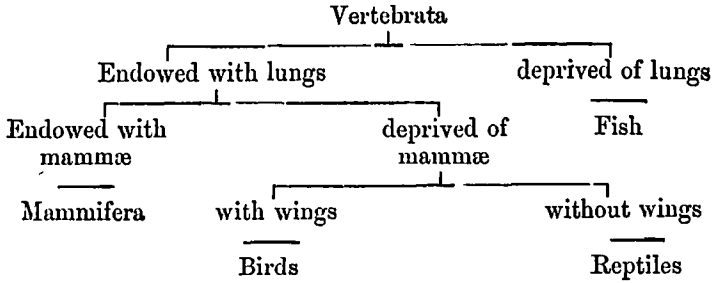
If, again, we divide the genus A into three species AB, AC, AD, we are either logically in error, or else we must be understood to imply the existence of three propositions excluding the union within the genus A of the properties of B, C and D, namely $AB = ABcd$, $AC = AbCd$, and $AD = AbcD$. It comes to the same thing if we say that our classification is really a bifurcate one, as follows :—



The logical necessity of bifurcate classification has been clearly and correctly stated in the 'Outline of a New System of Logic' by George Bentham, a work of which the logical value has been quite overlooked until lately. Mr. Bentham points out, in p. 113, that every classification must be essentially bifurcate and takes, as an example, the division of vertebrate animals into four subclasses, as follows :—

Mammifera—	endowed with mammæ and lungs.
Birds	without mammæ but with lungs and wings.
Fish	deprived of lungs.
Reptiles	deprived of mammæ and wings but with lungs.

We have, then, as Mr. Bentham says, three bifid divisions, thus represented:—



It is however quite evident that according to the laws of thought even this arrangement is incomplete. The subclass mammifera must either have wings or be deprived of them; we must subdivide this class, or assume that none of the mammifera have wings, which is, as a matter of fact, the case, the wings of bats not being true wings in the meaning of the term as applied to birds. Fish, again, ought to be considered with regard to the possession of mammæ and wings; and in leaving them undivided we really imply that they never have mammæ nor wings, the wings of the flying-fish, again, being no exception. If we resort to the use of our letters and define them as follows—

A = vertebrata,
B = having lungs,
C = having mammæ,
D = having wings,

then there are four existent classes of vertebrata which appear to be thus described—

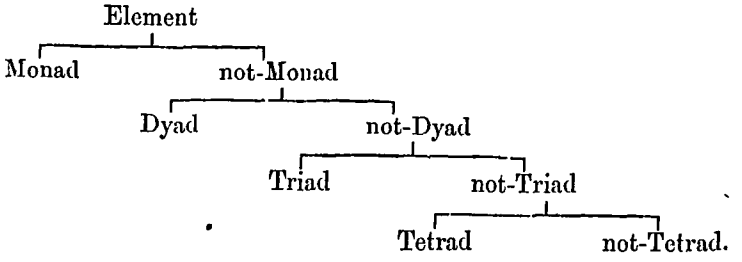
ABC
ABcD
ABc*d*
A*b*.

But in reality the combinations are implied to be

ABC*d* = Mammifera,
ABc*D* = Birds,
AB*cd* = Reptiles,
A*bcd* = Fish,

and we imply at the same time that the other four conceivable combinations containing B, C, or D, namely ABCD, A*b*CD, A*b*C*d*, and A*b*c*D*, do not exist in nature.

The bifurcate form of classification seems to be needless when the property according to which we classify any group of things admits of numerical discrimination. It would seem absurd to arrange things according as they have one degree of the property or not one degree, two degrees or not two degrees, and so on. The elements, for instance, are classified according as the atom of each saturates, one, two, three or more atoms of a monad element, such as chlorine, and they are called accordingly Monad, Dyad, Triad, Tetrad elements, and so on. It would be wholly useless to apply the bifid arrangement, thus :—



The reason of this is that, by the very nature of number as described in Chapter VIII, every number is logically discriminated from every other number. There can thus be no logical confusion in a numerical arrangement, and the series of numbers indefinitely extended is also exhaustive. Every thing admitting of a property expressible in numbers must find its place somewhere in the series of numbers. The chords in music correspond to the various simpler numerical ratios and must admit of complete

exhaustive classification in respect to the complexity of the ratios forming them. Plane rectilinear figures may also be classified according to the number of their sides as triangles, quadrilateral figures, pentagons, hexagons, heptagons, &c. The bifurcate arrangement is not false when applied to such series of objects; it is even necessarily involved in the arrangement which we do apply, so that its formal statement is needless and tedious. The same may be said of the division of portions of space. Reid and Kames endeavoured to cast ridicule on the bifurcate arrangement[n] by proposing to classify the parts of England into Middlesex and what is not Middlesex, dividing the latter again into Kent and what is not Kent, the latter again into Sussex and what is not Sussex; and so on. This is so far, however, from being an absurd proceeding that it is requisite to assure us that we have made an exhaustive enumeration of the parts of England.

The Five Predicables.

As a general rule it is highly desirable to consign to oblivion all the ancient logical names and expressions, which have infested the science for many centuries past. If logic is ever to be a useful and progressive science, logicians must distinguish between logic and the history of logic. As in the case of any other science it may be desirable to examine the course of thought by which logic has, before or since the time of Aristotle, been brought to its present state; the history of a science is always instructive as giving instances of the mode in which discoveries take place. But at the same time we ought carefully to disencumber the statement of the science

[n] George Bentham, 'Outline of a New System of Logic,' p. 115.

itself of all names and other vestiges of antiquity which are not actually useful at the present day.

Among those ancient expressions which may well be excepted from such considerations and ever retained in use, are the 'Five Words' or 'Five Predicables' which were described by Porphyry in his 'Introduction to Aristotle's Organum.' Two of them indeed, namely *Genus* and *Species*, are the most venerable names in philosophy, having probably been first employed in their present logical meanings by Socrates. In the present day it requires some mental effort, as Mr. Georges Lewes has remarked[o],' to see anything important in the invention of notions now so familiar as those of Genus and Species. But in reality the introduction of such terms showed the rise of the first germs of logic and scientific method: it showed that men were beginning to analyse their processes of thought.

The Five Predicables are Genus, Species, Difference, Property, and Accident, or in the original Greek γένος, εἶδος, διαφορὰ, ἴδιον, συμβεβηκός. Of these, Genus may be taken to mean any class of objects which is regarded as broken up into two minor classes, which form Species of it. The Genus is defined by a certain number of qualities or circumstances which belong to all objects included in the class, and which are sufficient to mark out these objects from all others which we do not intend to include. Interpreted as regards intension, then, the Genus is a group of qualities; interpreted as regards extension, it is a group of objects possessing those qualities. If now another quality be taken into account which is possessed by some of the objects and not by the others, this quality becomes a Difference which divides the Genus into two Species. We may interpret the Species

o 'Biographical History of Philosophy,' (1857) vol. i. p. 126. Grote's 'History of Greece,' vol. viii. p. 578.

either in intension or extension; in the former respect it is more than the Genus as containing one more quality, the Difference: in the latter respect it is less than the Genus as containing only a portion of the group constituting the Genus. We may say then, with Aristotle, that in one sense the Genus is in the Species, namely in intension, and in another sense the Species is in the Genus, namely in extension. The Difference, it is evident, can be interpreted in intension only.

A Property is a quality which belongs to the whole of a class, but does not enter into the definition of that class. Thus if it be a generic property it belongs to every individual object contained in the genus. It is a property of the genus Parallelogram that the opposite angles are equal. If we regard a Rectangle as a species of parallelogram, the difference being that *one* angle is a right angle, it follows as a specific property that all the angles are right angles. Though a property in the strict logical sense must belong to each of the objects included in the class of which it is a property, it may or may not belong to other objects. The property of having the opposite angles equal may belong to many figures besides parallelograms, for instance, regular hexagons. It is a property of the circle that all triangles constructed upon the diameter with the apex upon the circumference are right angled triangles, and *vice versâ*, all closed curves of which this is true must be circles. We might with advantage distinguish properties which thus belong to a class, and only to that class, as *peculiar properties*. They enable us to make statements in the form of simple identities (vol. i. p. 44). Thus we know it to be a peculiar property of the circle that for a given length of perimeter it encloses a greater area than any other possible curve; hence we may say—

Curve of equal curvature = curve of greatest area.

It is a peculiar property of equilateral triangles that they are equiangular, or, *vice versâ*, it is a peculiar property of equiangular triangles that they are equilateral. It is a property of crystals of the regular system that they are devoid of the power of double refraction, but this is not a property peculiar to them, because vitreous and other amorphous transparent solids, such as glass, together with all liquids and gases, are also devoid of the same property.

An Accident, the fifth and last of the Predicables, is any quality, which may or may not belong to certain objects, and which has no connexion with the classification adopted. The particular size of a crystal does not in the slightest degree affect the nature of the crystal, nor does the manner in which it may be grouped with other crystals; these, then, are Accidents as regards a crystallographic classification. With respect to the chemical composition of a substance, again, it is an accident whether the substance be crystallized or not, or whether it be organized or not. As regards botanical classification the absolute size of a plant is an accident, due to external circumstances. Thus we see that a logical accident is any quality or circumstance which is not known to be correlated with those qualities or circumstances forming the definition of the species.

The use of the Predicables can be very concisely explained by our symbols. Thus, let A be any definite group of qualities and B another quality; then A will constitute a genus, and AB, A*b* will be species of it, B being the difference. Let C, D and E be other qualities, and on examining the combinations in which A, B, C, D, E occur let them be as follows :—

ABCDE	A*b*C*d*E
ABCD*e*	A*b*C*de*.

Here we see that wherever A is C is also found, so that C is a generic property; D occurs always with B, so that it constitutes a specific property, while E is indifferently present and absent, so as not to be in any way correlated with any of the other letters; it represents, therefore, an accident. It will now be seen that the Logical Abecedarium really represents an interminable series of subordinate genera and species; it is but a concise symbolic statement of what was involved in the ancient doctrine of the Predicables.

Summum Genus and Infima Species.

As a genus means any class whatever which is regarded as composed of minor classes or species, it follows that the same class will be a genus in one point of view and a species in another. *Metal* is a genus as regards *alkaline metal*, a species as regards *element*, and any extensive system of classes consists of a series of subordinate, or as they are technically called, *subaltern* genera and species. The question, however, arises, whether any such chain of classes has a definite termination at either end. The doctrine of the old logicians was to the effect that it terminated upwards in a *genus generalissimum* or *summum genus*, which was not a species of any wider class. Some very general notion, such as *substance*, *object* or *thing*, was supposed to be so comprehensive as to include all thinkable objects, and for all practical purposes this might be so. But as I have already explained (vol. i. p. 88), we cannot really think of any object or class without thereby separating it from what is not that object or class. All thinking is relative, and implies discrimination, so that every class and every logical notion must have its negative. If so, there is no such thing as a *summum*

genus, for we cannot frame the requisite notion of a class forming it without implying the existence of another class discriminated from it, but which with the supposed *summum genus* will form the species of a still higher genus, which is absurd.

Although there is no absolute summum genus, nevertheless relatively to any branch of knowledge or any special argument, there is always some class or notion which bounds our horizon as it were. The chemist restricts his view to material substances and the forces manifested in them; the mathematician extends his view so as to comprehend all notions capable of numerical discrimination. The biologist, on the other hand, has a narrower sphere containing only organized bodies, and of these the botanist and the zoologist take parts. In other subjects there may be a still narrower summum genus, as when the lawyer regards only living and reasoning beings of his own country.

In the description of the Logical Abecedarium, it was pointed out (vol. i. p. 108) that every series of combinations was really the development of some one single class, denoted by X, which letter indeed was accordingly placed in the first column of the table on p. 109. This is the formal acknowledgment of the principle clearly stated by De Morgan, that all reasoning proceeds within some assumed summum genus. But at the same time the fact that X as a logical term must have its negative x, shows that it cannot be an absolute summum genus.

There arises, again, the question whether there be any such thing as an *infima species*, which cannot be divided into any smaller species. The ancient logicians were of opinion that there always was some assignable class which could only be divided into individuals, but this doctrine appears to me theoretically incorrect, as Mr. George

Bentham indeed long ago stated [p]. We may always put an arbitrary limit to the subdivisions of our classification at any point convenient to our purpose. The crystallographer would not generally consider as different species of crystalline form those which differ only in the degree of development of the faces. The naturalist overlooks innumerable slight differences between plants or animals which he refers to the same species. But in a strictly logical point of view classification might be carried on so long as there is a single point of difference, however minute, between two objects, and we might thus go on until we arrived at individual objects which are numerically distinct in the logical sense attributed to that expression in the chapter upon Number. We must either, then, call the individual the *infima species* or allow that there is no such species at all.

The Tree of Porphyry.

The bifurcate method of classification, arising as it does from the primary laws of thought, is the very foundation of all strict scientific method, and its application in formal logic constitutes the method of Indirect Inference, of which the nature and importance were shown in Chapter VI. So slight, however, has been the attention paid to this all important subject, that I shall in this case break the rule which I have laid down for myself, not to mingle the subject of logic as a science with the history of logic.

Both Plato and Aristotle were fully acquainted with the value of bifurcate division which they occasionally employed in an explicit manner. It is impossible, too,

[p] 'Outline of a New System of Logic,' 1827, p. 117.

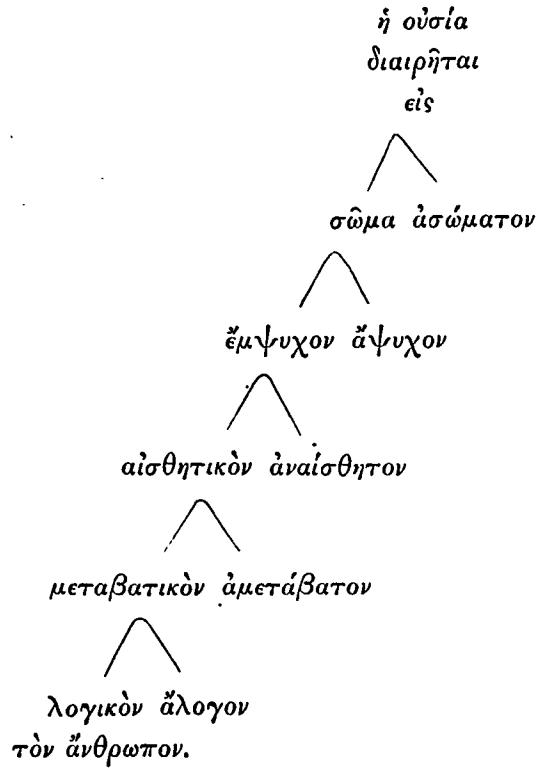
that Aristotle should state the laws of thought, and employ the predicables without implicitly recognising the logical necessity of that method. It is, however, in Porphyry's remarkable and in many respects excellent 'Introduction to the Categories of Aristotle' that we find the most distinct account of it. Porphyry not only fully and accurately describes the Predicables, but incidently introduces an example for illustrating those predicables, which constitutes a good specimen of bifurcate classification. Translating his words[q] freely we may say that he takes Substance as the genus to be divided, under which are successively placed as Species—Body, Animated Body, Animal, Rational Animal, and Man. Under Man, again, come Socrates, Plato, and other particular men. Now of these notions Substance is the genus generalissimum, and is a genus only, not a species. Man, on the other hand, is the species specialissima (infima species), and is a species only, not a genus. Body is a species of substance, but a genus of animated body, which, again, is a species of body but a genus of animal. Animal is a species of animated body, but a genus of rational animal, which, again, is a species of animal, but a genus of man. Finally, man is a species of rational animal, but is a species merely and not a genus, being divisible only into particular men.

Porphyry proceeds at some length to employ his example in further illustration of the predicables. We do not find in Porphyry's own work any scheme or diagram exhibiting this curious specimen of classification, but some of the earlier commentators and epitome writers drew what has long been called the Tree of Porphyry.

Thus in the 'Epitome Logica' of Nicephorus Blemmidas,

[q] 'Porphyrii Isagoge,' Caput ii. 24.

we find a diagram[r] of which the following is nearly a facsimile :—

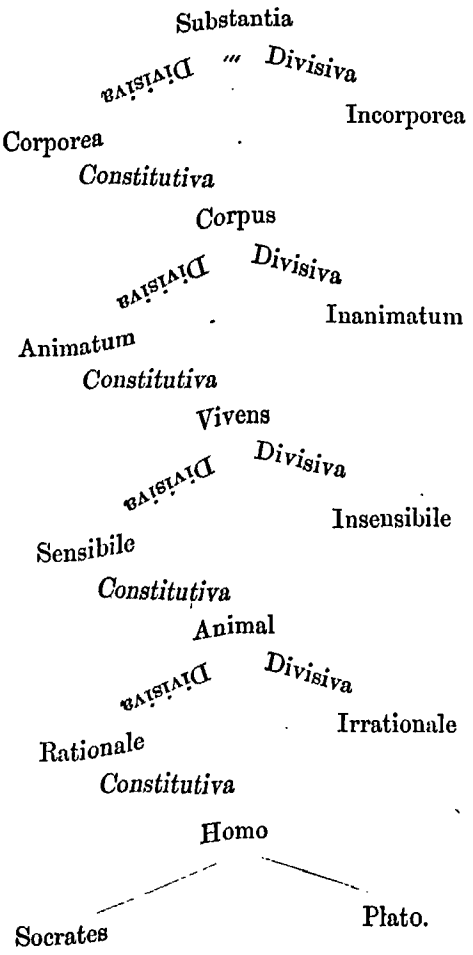


In the above scheme we find the bifurcate principle accurately but not completely applied. Each genus is subdivided into two species, described by a pair of positive and negative terms, so that the species are together equal in extent to the genus. But it will of course be observed that each negative branch is left without further subdivision, so that there is only a single infima species, namely man, instead of thirty-two final branches, as there would be in a theoretically complete system.

This tree was subsequently reproduced in the works of a multitude of logicians in a form which is more complicated and not so good as that of Nicephorus. Thus

[r] 'Epitome Logica, Augustæ Vindel.' 1605, p. 118.

in the 'Opuscula' of Aquinas, as quoted by Mansel in his edition of Aldrich's 'Artis Logicæ Rudimenta,' second edition, p. 31, we find the Tree nearly in the following form:



This example of the bifurcate method, although repeated in almost all compendiums and treatises on logic, attracted no particular attention until the time of Peter Ramus and his followers, who are commonly said to have bestowed so much attention and praise upon it as to be

regarded by some persons as its inventors. The Ramean Tree is a name frequently employed instead of the Porphyrian Tree, or the κλῖμαξ, that is, the Ladder of Porphyry, as it was sometimes called by the Greek logicians. Although I have looked through several commentaries upon the Dialectics of Ramus, I do not find that very much is said upon the subject. In the Questions of Frederick Beurhusius[r], the method of dichotomy is described as 'illa naturalis et antiquissimorum philosophorum præstantissima Dichotomia,' but in none of the works do I find the Tree itself given.

Among modern logicians Jeremy Bentham possesses the great merit of having drawn attention to the logical importance of bifurcate division. His remarks on the subject are contained in that extraordinary collection of digressive, and often almost incomprehensible papers, called Chrestomathia[s], two of the formidable title-pages of which are given below. The fifth appendix in this work, forming the larger and most important part of the book, consists of an Essay on Nomenclature and Classification[t]. Although written in his later and worse style, this essay is well worth reading, and full of forcible remarks. It may be regarded, I believe, as the first of

[r] In Petri Rami, Regii Professoris Clariss. Dialecticæ Libros duos Lutetiæ Anno LXXII, postremo sine Prælectionibus æditos, explicationum Quæstiones: quæ Prædagogiæ Logicæ de Docenda Discendaque Dialectica. Auctore Frederico Beurhusio. Londoni, 1581, p. 120.

[s] 'Chrestomathia: being a Collection of Papers, explanatory of the Design of an Institution proposed to be set on foot, under the name of the Chrestomathic Day School, or Chrestomathic School, for the extension of the New System of Instruction,' &c. By Jeremy Bentham, Esq., London, 1816.

[t] 'An Essay on Nomenclature and Classification: including a Critical Examination of the Encyclopædical Table of Lord Bacon, as improved by D'Alembert: and the first lines of a new one grounded on the application of the Logical Principle of Exhaustively Bifurcate Analysis.' London, 1817.

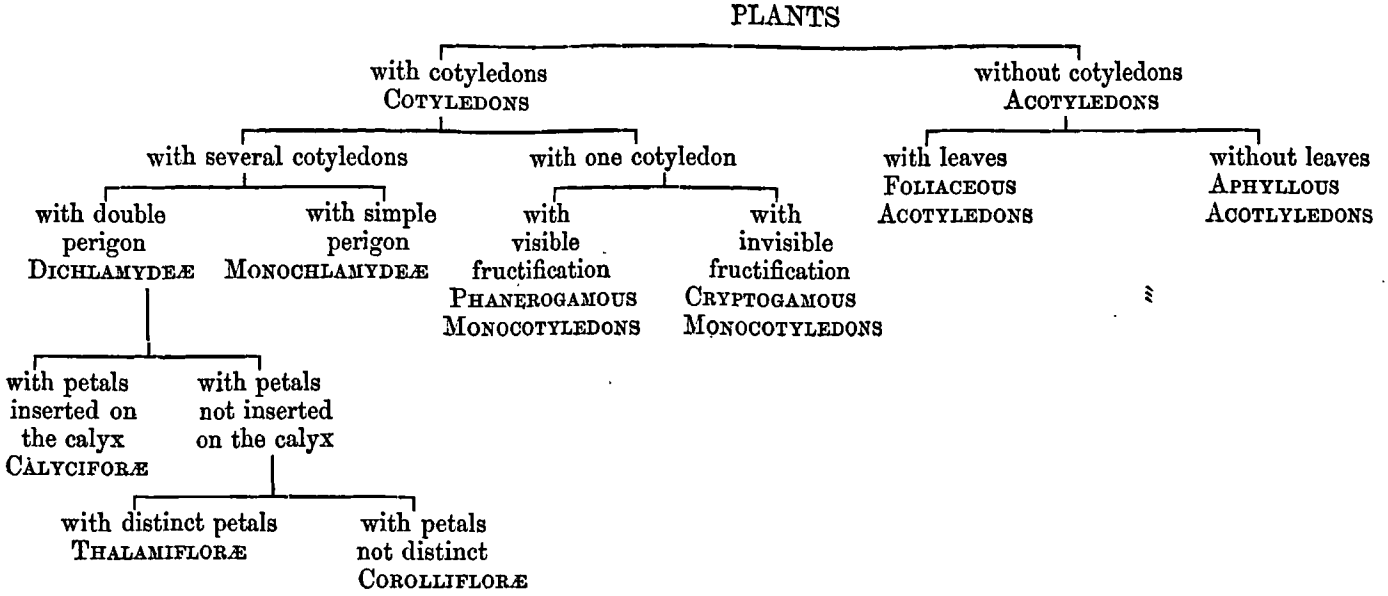
the series of English writings which have, in the present century, made logic a new and progressive science. In Table IV. Bentham gives the Arbor Porphyriana, as exhibited in the course of a college lecture in 1761, calling it the original form. His reading of logic seems to have been restricted to the compendiums of Saunderson and Watts, and it was only after the text was written that he obtained an opportunity of consulting the work of Porphyry, and was surprised to find no diagram therein. He attributes its invention to Peter Ramus, although he had never seen the writings of that logician, and had merely learnt their titles from a dictionary.

In this essay he states in the most powerful way the advantages of the bifurcate method of classification, which had been suggested to him by a chapter in Saunderson's logic and the diagram given in the college course. Although the Tree of Porphyry and the principles of bifurcation had been mentioned by almost all logicians, the utility and excellence of the method, he says (p. 287), had not made itself apparent. Indeed the method was mentioned but to be slighted, or to be made a subject of pleasantry by Reid and Kames. Bentham sufficiently states his own opinion when he speaks (p. 295) of 'the matchless beauty of the Ramean Tree.' After fully showing its logical value as an exhaustive method of classification, and refuting the objections of Reid and Kames, on a wrong ground, as I think, he proceeds to inquire to what length it may be carried. He correctly points out two objections to the extensive use of bifid arrangements, (1) because they soon become impracticably extensive and unwieldy, and (2) because they are uneconomical. In his day the recorded number of different species of plants was 40,000, and he leaves the reader to estimate the immense number of branches and the enormous area of a bifurcate table which should exhibit all these species in

one scheme. He also points out the apparent loss of labour in making any large bifurcate classification; but this he considers to be fully recompensed by the logical value of the result, and the logical training acquired in its execution. Jeremy Bentham, then, fully recognises, as I conceive, the value of the Logical Abecedarium under another name, though he apprehends the limit to its use placed by the finiteness of our mental and manual powers.

Mr. George Bentham has also fully recognised the value of bifurcate classification, both in his 'Outline of a New System of Logic'[u] (pp. 105–118), and in his 'Essai sur la Nomenclature et la Classification.' This latter work consists of a free translation or improved version in French of Jeremy Bentham's 'Essay on Classification.' Further illustrations of the value of the bifurcate method are adduced from the natural sciences, and Mr. Bentham points out that it is really this method which was employed by Lamark and Decandolle in their so-called analytical arrangement of the French Flora. The following table contains an excellent example of bifurcate division, consisting of the principal classes of Decandolle's system, as given by Mr. Bentham in Table No. III. p. 108 of his Essay, the names, however, being translated:—

[u] Concerning the connexion of this work with the great discovery of the quantification of the predicate, I may refer the reader to the remarks and articles of Mr. Herbert Spencer and Professor Thomas Spencer Baynes, in the 'Contemporary Review' of March, April, and July, 1873, vol. xxi. pp. 490, 796; vol. xxii. p. 318; as also to my own article in answer to Professor Baynes in the same Review for May, 1873, vol. xxi. p. 821. Professor Baynes makes it evident that, when Sir W. Hamilton reviewed Mr. Bentham's work in 1833, he did not sufficiently acquaint himself with its contents. I must continue to hold that the principle of quantification is explicitly stated by Mr. Bentham, and it must be regarded as a remarkable fact in the history of logic that Hamilton, while vindicating, in 1847, his own claims to originality and priority against the scheme of De Morgan, should have overlooked the much earlier and more closely related discoveries of Bentham.


PLANTS
with cotyledons
COTYLEDONS
without cotyledons
ACOTYLEDONS
with several cotyledons
with one cotyledon
with leaves
FOLIACEOUS
ACOTYLEDONS
without leaves
APHYLLOUS
ACOTLYLEDONS
with double perigon
DICHLAMYDEÆ
with simple perigon
MONOCHLAMYDEÆ
with visible fructification
PHANEROGAMOUS
MONOCOTYLEDONS
with invisible fructification
CRYPTOGAMOUS
MONOCOTYLEDONS
with petals inserted on the calyx
CÀLYCIFORÆ
with petals not inserted on the calyx
with distinct petals
THALAMIFLORÆ
with petals not distinct
COROLLIFLORÆ

Mr. Bentham also gives a bifurcate arrangement of animals after the method proposed by Duméril in his 'Zoologie Analytique,' this naturalist being distinguished by his clear perception of the logical importance of the method.

A more recent binary classification of the animal kingdom as regards the larger classes may be found in Professor Reay Greene's 'Manual of the Cœlenterata,' p. 18.

Does Abstraction imply Generalization?

Before we can acquire a sound comprehension of the subject of classification we must answer a very difficult question, namely, whether logical abstraction does or does not always imply generalization. It comes to exactly the same thing if we ask whether a species may be coextensive with its genus, or whether, on the other hand, the genus must contain more than the species. To abstract logically is, as we have seen (vol. i. p. 33), to overlook or withdraw our notice from some point of difference. Whenever we form a class we abstract, for the time being, the differences of the objects so united in respect of some common quality. If, for instance, we class together a great number of objects as dwelling-houses, we overlook or abstract the fact that some dwelling-houses are constructed of stone, others of brick, wood, iron, &c. Very often at least the abstraction of a circumstance increases the number of objects included under a class according to the law of the inverse relation of the quantities of extension and intension (vol. i. p. 32). Dwelling-house is a wider term than brick dwelling-house. House, or building, is more general still than dwelling-house. But the question before us is, whether abstraction *always* increases the number of objects included in a class, which amounts to asking whether the law of the inverse relation of logical quantities is *always* true. The interest of the question

partly arises from the fact, that so high a philosophical authority as Mr. Herbert Spencer has denied that generalization is implied in abstraction[x], making this doctrine the ground for rejecting previous methods of classifying the sciences, and for forming an ingenious but peculiar method of his own. The question is also a fundamental one of the highest logical importance, and involves subtle difficulties which have made me long hesitate in forming a decisive opinion.

Let us attempt to answer the question by examination of a few examples. Compare the two classes *gun* and *iron gun*. It is certain that there are many guns which are not made of iron, so that abstraction of the circumstance 'made of iron' increases the extent of the notion. Next compare *gun* and *metallic gun*. All guns made at the present day consist, I believe, of metal, so that the two notions seem to be co-extensive; but guns were at first made of pieces of wood bound together like a tub, and as the logical term *gun* takes no account of time, it must include all guns that have ever existed. Here again extension increases as intension decreases. Compare once more 'steam-locomotive engine' and 'locomotive engine.' In the present day so far as I am aware all locomotives are worked by steam, so that the omission of that qualification might seem not to widen the term; but it is quite possible that in some future age a different motive power may be used in locomotives; and as there is no limitation of time in the use of logical terms, we must certainly assume that there is a class of locomotives not worked by steam, as well as a class that is worked by steam. When the natural class of Euphorbiaceæ was originally formed, all the plants known to belong to it were devoid of corollas; it would have seemed therefore that the two classes 'Euphorbiaceæ,' and 'Euphorbiaceæ devoid

x 'The Classification of the Sciences,' &c., 3rd edit. p. 7.

of Corollas,' were of equal extent. Subsequently a number of plants plainly belonging to the same class were found in tropical countries, and they possessed bright coloured corollas. Naturalists believe with the utmost confidence that 'Ruminants' and 'Ruminants with cleft feet' are identical terms, because no ruminant has yet been discovered without cleft feet. But we can see no impossibility in the conjunction of rumination with uncleft feet, and it would be too great an assumption to say that we are certain that an example of it will never be met with. Instances can be quoted, without end, of objects being ultimately discovered which combined properties or forms which had never before been seen together. In the animal kingdom the Black Swan, the Ornithorhyncus Paradoxus, and more recently the singular fish called Ceratodus Forsteri, all discovered in Australia, have united characters never previously known to co-exist. At the present time deep-sea dredging is bringing to light many animals of a new and unprecedented nature. Singular exceptional discoveries may certainly occur in other branches of science. When Davy first succeeded in eliminating metallic potassium, it was a well established empirical law that all metallic substances possessed a high specific gravity, the least dense of all metals then known being zinc, of which the specific gravity is 7·1. Yet, to the surprise of chemists, potassium was found to be an undoubted metal of less density than water, its specific gravity being 0·865.

It is hardly requisite to prove by further examples that our knowledge of nature is incomplete, so that we cannot safely assume the non-existence of new combinations. Logically speaking, we ought to leave a place open for animals which ruminate but are without cleft feet, and for every other possible intermediate form of animal, plant, or mineral. A purely logical classification must take account not only of what certainly does exist, but of what may in after ages be found to exist.

I will go a step further, and say that we must have places in our scientific classifications for purely imaginary existences. A very large proportion of the mathematical functions which are conceivable have no application to the circumstances of this world. Physicists certainly do investigate the nature and consequences of forces which nowhere exist. Newton's 'Principia' is full of such investigations. In one chapter of his 'Mécanique Céleste' Laplace indulges in a remarkable speculation as to what the laws of motion would have been if momentum instead of varying simply as the velocity had been a more complicated function of it. I have already mentioned (vol. i. p. 256) that Sir George Airy contemplated the existence of a world in which the laws of force should be such that a perpetual motion would be possible, and the Law of Conservation of Energy would not hold true.

Thought is not bound down to the limits of what is materially existent, but is circumscribed only by those Fundamental Laws of Identity, Contradiction and Duality, which were laid down at the outset. This is the point at which I should differ from Mr. Herbert Spencer. He appears to suppose that a classification is complete if it has a place for every existing object, and this may perhaps seem to be practically sufficient; but it is subject to two profound objections. Firstly, we do not know all that exists, and therefore in limiting our classes we are erroneously omitting multitudes of objects of unknown form and nature which may exist either on this earth or in other parts of space. Secondly, as I have explained, the powers of thought are not limited by material existences, and we may or, for some purposes, must imagine objects which probably do not exist, and if we imagine them we ought (strictly speaking) to find appropriate places for them in the classifications of science.

The chief difficulty of this subject, however, consists in

the fact that mathematical or other certain laws may entirely forbid the existence of some combinations. The circle may be defined as a plane curve of equal curvature, and it is a property of it that it contains the greatest area within the least possible perimeter. May we then contemplate mentally a circle not a figure of greatest possible area? Or, to take a still simpler example, a parallelogram possesses the property of having the opposite angles equal. May we then mentally divide parallelograms into two classes according as they do or do not have their opposite angles equal? It might seem absurd to do so, because we know that one of the two species of parallelogram would be non-existent. But, then, what is the meaning of the thirty-fourth proposition of Euclid's first book, unless the student had previously contemplated the existence of both species as possible. We cannot even deny or disprove the existence of a certain combination without thereby in a certain way recognising that combination as an object of thought.

The general conclusion, then, at which I arrive, is in opposition to that of Mr. Herbert Spencer. I think that whenever we abstract a quality or circumstance we do generalize or widen the notion from which we abstract. Whatever the terms A, B, and C may be, I hold that in strict logic AB is mentally a wider term than ABC, because AB includes the two species ABC and AB*c*. The term A is wider still, for it includes the four species ABC, AB*c*, A*b*C, A*bc*. The Logical Abecedarium, in short, is the only limit of the classes of objects which we must contemplate in a purely logical point of view. Whatever notions be brought before us, we must mentally combine them in all the ways sanctioned by the laws of thought and exhibited in the Abecedarium, and it is a matter for after consideration to determine how many of these combinations exist in outward nature, or how many are actually

forbidden by the nature of space. A classification is essentially a mental not a material thing.

Discovery of Marks or Characteristics.

Although the chief purpose of classification is to disclose the deepest and most general resemblances of the objects classified, yet the practical value of any particular system will partly depend upon the ease with which we can refer an object to its proper class, and thus infer concerning it all that is known generally of that class. This operation of discovering to which class of a system a certain specimen or case belongs is generally called *Diagnosis*, a technical term very familiarly used by physicians, who constantly require to diagnose or determine the nature of the disease from which a patient is suffering. Now every class is defined by certain specified qualities or circumstances, the whole of which are present in every object contained in the class, and not all present in any object excluded from it. These defining circumstances ought to consist of the deepest and most important circumstances, by which we vaguely mean those probably forming the conditions with which the minor circumstances are correlated. But it will often happen that the so-called important points of an object are not those which can most readily be observed. Thus the two great classes of phanerogamous plants are defined respectively by the possession of two cotyledons or seed-leaves, and one cotyledon. But when a plant comes to our notice and we want to refer it to the right class, it will often happen that we have no seed at all to examine, in order to discover whether there be one seed-leaf or two in the germ. Even if we have a seed it will often be very small, and a careful dissection under the microscope will be requisite to ascertain the number of cotyledons. Occasionally the

examination of the germ would mislead us, for the cotyledons may be obsolete, as in Cuscuta, or united together, as in Clintonia. Botanists therefore seldom actually refer to the seed for such simple information. Certain other characters of a plant are closely correlated with the number of seed-leaves; thus monocotyledonous plants almost always possess leaves with parallel veins like those of grass, while dicotyledonous plants have leaves with reticulated veins like those of an oak leaf. In monocotyledonous plants, too, the parts of the flower are most often three or some multiple of three in number, while in dicotyledonous plants the numbers four and five and their multiples prevail. Botanists, therefore, by a glance at the leaves and flowers can almost certainly refer a plant to its right class, and can infer not only the number of cotyledons which would be found in the seed or young plant, but also the structure of the stem and the other general characters and relations of a dicotyledon or a monocotyledon.

Any conspicuous and easily discriminated property which we thus select for the purpose of deciding to which class an object belongs, may be called a *characteristic*. The logical conditions of a good characteristic mark are very simple, namely, that it should be possessed by all objects entering into a certain class, and by none others. The characteristic may consist either of a single quality or circumstance, or of a conjunction of such, provided that they all be constant and easily detected. Thus in the classification of mammals the teeth are of the greatest assistance, not because a slight variation in the number and form of the teeth is of any great importance in the general economy of the animal, but because such variations are found by empirical observation to coincide with most important differences in the general affinities. It is found that the minor classes and genera of mammals can be

registered and discriminated accurately by their teeth, especially by the foremost molars and the hindmost premolars. Some of the teeth, indeed, are occasionally missing, so that zoologists prefer to trust to those characteristic teeth which are most constant[y], and to infer from them not only the arrangement of the other teeth, but the whole conformation of the animal.

It is a very difficult matter to mark out any boundary-line between the animal and vegetable kingdoms, and it may even be doubted whether any rigorous division can be established. The most fundamental and important character of a vegetable structure probably consists in the absence of nitrogen from the constituent membranes. Supposing this to be the case, the difficulty arises that in examining minute organisms we cannot ascertain directly whether they contain nitrogen or not. Some minor but easily detected circumstance is therefore needed to discriminate between animals and vegetables, and this is furnished to some extent by the fact that the production of starch granules is restricted to the vegetable kingdom. Thus the Desmidiaceæ may be safely assigned to the vegetable kingdom, because they contain starch. But we must not employ this characteristic negatively; the Diatomaceæ are probably vegetables, though they do not produce starch.

Diagnostic Systems of Classification.

We have seen that diagnosis is the process of discovering the place in any system of classes, to which an object has already been referred by some previous investigation, the object being to avail ourselves of the information concerning such an object which has been already accumulated and recorded. It is obvious that this is a

[y] Owen, 'Essay on the Classification and Geographical Distribution of the Mammalia,' p. 20.

matter of the greatest importance, for, unless we can recognise, from time to time, objects or substances which have been before investigated, all recorded discoveries would lose their value. Even a single investigator must have some means of recording or systematizing his observations of any large number of objects like those furnished by the vegetable and animal kingdoms.

Now whenever a class has been properly formed, a definition must have been laid down, stating the qualities and circumstances possessed by all the objects which are intended to be included in the class, and not possessed completely by any other objects. Diagnosis, therefore, consists simply in comparing the qualities of a certain object with the definitions of a series of classes; the absence in the object of any one quality stated in the definition excludes it from the class thus defined; whereas, if we find every point of a definition exactly fulfilled in the specimen, we may at once assign it to the class in question. It is of course by no means certain that everything which has been affirmed of a class is true of all objects afterwards referred to the class; for this would be a case of imperfect inference, which is never more than a matter of probability. A definition can only make known a finite number of the properties of an object, so that it always remains possible that objects agreeing in those assigned properties will differ in other ones. An individual cannot be defined, and can only be made known by the exhibition of the individual itself, or by a material specimen exactly representing it. But this and many other questions relating to definition must be treated if I am able to take up the general subject of language in another work.

Diagnostic systems of classification should, as a general rule, be arranged on the bifurcate method explicitly. Any property may be chosen which divides the whole group

of objects into two distinct parts, and each part may be sub-divided successively by any prominent and well marked circumstance which is present in a large part of the genus and not in the other. To refer an object to its proper place in such an arrangement we have only to note whether it does or does not possess the successsive critical circumstances. Dana devised a classification of this kind[z] by which to refer any crystal to its place in the series of six or seven classes already described. If a crystal has all its edges modified alike or the angles replaced by three or six similar planes, it belongs to the monometric system; if not, we observe whether the number of similar planes at the extremity of the crystal is three or some multiple of three, in which case it is a crystal of the hexagonal system; and so we proceed with further successive discriminations.

To ascertain the name of a mineral by examination with the blow-pipe, an arrangement more or less evidently on the bifurcate plan, has been laid down by Von Kobell[a]. Minerals are divided according as they possess or do not possess metallic lustre; as they are fusible (including under fusible substances those which are volatile) or not fusible in a determinate degree, according as they do or do not on charcoal give a metallic bead, and so on.

Perhaps the best example to be found of any arrangement simply devised for the purpose of diagnosis, is Mr. George Bentham's 'Analytical Key to the Natural Orders and Anamolous Genera of the British Flora,' given in his 'Handbook of the British Flora[b].' In this

[z] Dana's 'Mineralogy,' vol. i. p. 123. Quoted in Watts's 'Dictionary of Chemistry,' vol. ii. p. 166.

[a] 'Instructions for the Discrimination of Minerals by Simple Chemical Experiments,' by Franz von Kobell, translated from the German by R. C. Campbell, Glasgow, 1841.

[b] Edition of 1866, p. lxiii.

scheme, the great composite family of plants, together with the closely approximate genus Jasione, are first separated from all other flowering plants by the compound character of their flowers. The remaining plants are subdivided according as the perianth is double or single. Since no plants are yet known in which the perianth can be said to have three or more distinct rings, this division becomes practically the same as one into double and not-double. Flowers with a double perianth are next discriminated according as the corolla does or does not consist of one piece, according as the ovary is free or not-free, as it is simple or not simple, as the corolla is regular or irregular, and so on. On looking over this arrangement, it will be found that numerical discriminations often occur, the numbers of petals, stamens, capsules, or other parts being the criteria, in which cases, as already explained (vol. ii. p. 374), the actual exhibition of the bifid division would be tedious.

Linnæus appears to have been perfectly acquainted with the nature and uses of diagnostic classification, which he describes under the name of Synopsis, saying[c]:—'Synopsis tradit Divisiones arbitrarias, longiores aut breviores, plures aut pauciores: a Botanicis in genere non agnoscenda. Synopsis est dichotomia arbitraria, quæ instar viæ ad Botanicem ducit. Limites autem non determinat.'

The rules and tables drawn out by chemists to facilitate the discovery of the nature of a substance in qualitative analysis are usually arranged on the bifurcate method, and form excellent examples of diagnostic classification, the qualities of the substances employed in testing being in most cases merely characteristic properties of little importance in other respects. The chemist does not detect potassium by reducing it to the state of metallic potas-

[c] 'Philosophia Botanica' (1770), § 154, p. 98.

sium, and then observing whether it has all the principal qualities belonging to potassium. He selects from among the whole number of compounds of potassium that salt, namely the compound of platinum tetra-chloride and potassium chloride, which has the most distinctive appearance, as it is comparatively insoluble and produces a peculiar yellow and highly crystalline precipitate. Accordingly whenever this precipitate can be produced by adding platinum chloride to a solution potassium is present. The fine purple or violet colour which potassium salts usually communicate to the blowpipe flame, had long been used as a characteristic mark. Some other elements were readily detected by the colouring of the blowpipe flame, barium giving a pale yellowish green, and salts of strontium a bright red. By the use of the spectroscope the coloured light given off by any incandescent vapour is made to give perfectly characteristic marks of the elements contained in the vapour.

Diagnosis seems to be identical with the process termed by the ancient logicians *abscissio infiniti*, the cutting off of the infinite or negative part of a classification when we discover by observation that an object possesses a particular property. At every step in a bifurcate division, some objects possessing the difference will fall into the affirmative part or species; all the remaining objects in the world fall into the negative part which will be infinite in extent. Diagnosis consists in the successive rejection from further notice of those almost infinite classes with which the specimen in question does not agree.

Index Classifications.

Under the general subject of classification we may certainly include all arrangements of objects or names, which we make for the purpose of saving labour in the

discovery of an object. Even such apparently trivial and arbitrary arrangements as alphabetical or other indices, are really classifications subject to all the principles of the subject. No such arrangement can be of any use unless it involves some correlation of circumstances, so that knowing one thing we learn another. If we merely arrange letters in the pigeon-holes of a secretaire we establish a correlation, for all letters in the first hole will be written by persons, for instance, whose names begin with A, and so on. Knowing then the initial letter of the writer's name we know also the place of the letter, and the labour of search is thus reduced to one twenty-sixth part of what it would be without any arrangement.

Now the purpose of a mere catalogue is to discover the place in which an object is to be found, but the art of cataloguing involves logical considerations of some interest and importance. We want to establish a correlation between the place of an object and some circumstance about the object which shall enable us readily to refer to it; this circumstance therefore should be that which will most readily dwell in the memory of the searcher. A piece of poetry, for instance, will be best remembered, in all probability, by the first line of the piece, according to the laws of the association of ideas, and the name of the author will be the next most definite circumstance; a catalogue of poetry should therefore be arranged alphabetically according to the first word of the piece, or the name of the author, or, still better, in both ways. It would be wholly absurd and impossible to arrange poems according to their subjects, so vague and mixed are these found to be when the attempt is made.

It is a matter of considerable literary importance to decide upon the best mode of cataloguing books, so that any required book in a library shall be most readily found. Books may be classified in a great number of

ways, according to subject, language, date or place of publication, size, the initial words of the book itself, of the title-page, the colophon, the author's name, the publisher's name, the printer's name, the character of the type, and so on. Every one of these modes of arrangement may be useful, for we may happen to remember one circumstance about a book, when we have forgotten all others; but as we cannot usually go to the expense of forming more than two or three indices at the most, we must of course select those circumstances for the basis of arrangement which will be likely to lead to the discovery of a book most surely. Many of the criteria mentioned are evidently inapplicable. The language in which a book is written is no doubt definite enough, but would afford no criterion for the classification of any large group of English books, or of those written in any one language. Classification by subjects would be an exceedingly useful method if it were practicable, but experience, or indeed a little reflection, shows it to be a logical absurdity. It is a very difficult matter to classify the sciences, so close and complicated are in many cases the relations between them. But with books the complication is infinitely greater, since the same book may treat successively of different sciences, or it may discuss a problem involving many entirely diverse principles and branches of knowledge. A good history of the steam engine will be antiquarian, so far as it traces out records of the earliest efforts at discovery; purely scientific, as regards the principles of thermodynamics involved; technical, as regards the mechanical means of applying those principles; economical, as regards the industrial results of the invention; biographical, as regards the lives of the inventors. A history of Westminster Abbey might belong either to the history of architecture, the history of the church, or the history of England. If we

abandon the attempt to carry out an arrangement according to the natural classification of the sciences, and form comprehensive practical groups, we shall be continually perplexed by the occurrence of intermediate cases, and opinions will differ *ad infinitum* as to the details. If, to avoid the difficulty about Westminster Abbey, we form a class of books devoted to the History of Buildings, the question will then arise whether Stonehenge is a building, and if so, whether, cromlechs, mounds, or even monoliths are so. At the other end of the scale we shall be uncertain whether to include under the class History of Buildings, lighthouses, monuments, bridges, &c. In regard to purely literary works, rigorous classification is still less possible. The very same work may partake of the nature of poetry, biography, history, philosophy, or if we form a comprehensive class of Belles-Lettres, nobody can say exactly what does or does not come under the term.

My own experience entirely bears out the opinion of the late Professor De Morgan, that classification according to the name of the author is the only one practicable in a large library, and this method has been admirably carried out in the great Catalogue of the British Museum. The name of the author is the most precise circumstance concerning a book, which usually dwells in the memory. It is more nearly a characteristic of the book than anything else. In an alphabetical arrangement we have an exhaustive classification, including a place for every possible name. The following remarks[d] of De Morgan seem therefore to be entirely correct. 'From much, almost daily use, of catalogues for many years, I am perfectly satisfied that a classed catalogue is more difficult to use than to make. It is one man's theory of the subdivision of knowledge, and the chances are against its suiting any other man. Even if all doubtful works were entered under several

[d] 'Philosophical Magazine,' 3rd Series (1845), vol. xxvi. p. 522.

different heads, the frontier of the dubious region would itself be a mere matter of doubt. I never turn from a classed catalogue to an alphabetical one without a feeling of relief and security. With the latter I can always, by taking proper pains, make a library yield its utmost; with the former I can never be satisfied that I have taken proper pains, until I have made it, in fact, as many different catalogues as there are different headings, with separate trouble for each. Those to whom bibliographical research is familiar, know that they have much more frequently to hunt an author than a subject: they know also that in searching for a subject, it is never safe to take another person's view, however good, of the limits of that subject with reference to their own particular purposes.'

It is often very desirable, however, that an alphabetical name catalogue should be accompanied by a subordinate subject catalogue, but in this case no attempt should be made to devise a theoretically complete classification. Every principal subject treated in a book should be entered separately in an alphabetical list, under the name most likely to occur to the searcher, or under several names. This method was partially carried out in Watts's valuable 'Bibliotheca Britannica,' but it was perfectly applied in the admirable subject index to the 'British Catalogue of Books,' and equally well in the 'Catalogue of the Manchester Free Library at Campfield,' this latter being the most perfect model of a printed catalogue with which I am acquainted. The public Catalogue of the British Museum is arranged as far as possible according to the alphabetical order of the author's names, but in writing the titles for this catalogue several copies are simultaneously produced by a manifold writer, so that a catalogue according to the order of the books on the shelves, and another according to the first words of the title-page, are created by a mere re-

arrangement of the spare copies. In the 'English Cyclopædia' it is suggested that twenty copies of the book titles might readily have been utilized in forming additional catalogues, arranged according to the place of publication, the language of the book, the general nature of the subject, and so forth [e].

It will hardly be a digression to point out the enormous saving of labour, or, what comes to the same thing, the enormous increase in our available knowledge, both literary and scientific, which arises from the formation of extensive indices. The 'State Papers,' containing the whole history of the nation, were practically sealed to literary inquirers until the Government undertook the task of calendaring and indexing them. The British Museum Catalogue is another national work, of which the importance in advancing knowledge cannot be overrated. The Royal Society is accomplishing a work of world-wide importance, in publishing a complete catalogue of memoirs upon physical science. The time will perhaps come when our views upon this subject will be extended, and either Government or some public society will undertake the systematic cataloguing and indexing of masses of historical and scientific information which are now almost closed against inquiry.

Classification in the Biological Sciences.

The great generalizations established in the works of Herbert Spencer and Charles Darwin have thrown great light upon many other sciences, and, strange as it may seem to say so, they have removed several difficulties out of the way of the logician. The subject of classification has long been studied in almost exclusive reference to the

[e] 'English Cyclopædia,' 'Arts and Sciences,' vol. v. p. 233.

arrangement of the various kinds of animals and plants. Systematic Botany and Zoology have been commonly known as the Classificatory Sciences, and scientific men seemed to suppose that the methods of arrangement, which we e suitable for living creatures, must be the best for all other classes of objects. Several mineralogists, especially Mohs, have attempted to arrange minerals in genera and species, just as if they had been animals capable of reproducing their kind with variations, and thus having relatives like distant cousins.

It is highly remarkable that this confusion of ideas between the relationship of living forms and the logical relationship of things in general prevailed from the earliest times, as manifested in the etymology of words. We familiarly speak of a *kind* of things meaning a class of things, and the kind consists of those things which are *akin*, or come of the same race. It is even believed by some etymologists that *second* means *other kind*, the Latin suffix *cund* being thus regarded as cognate with *kind*[f]. Similarly when Socrates and his followers wanted a name for a class regarded in a philosophical light, they again adopted the analogy in question, and called it a γενος, or race, the root γεν- being distinctly connected with the notion of generation.

So long as the species of plants and animals were believed to proceed from distinct and unconnected acts of Creation, the multitudinous points of resemblance and difference which they present, possessed a simply logical character, and might be treated as a guide to the classification of other objects generally. But when once we come to regard these resemblances as purely hereditary in their origin, we see that the sciences of systematic Botany and Zoology have a special character of their own. There is no reason whatever to suppose that the

[f] Vernon, 'Anglo-Saxon Guide,' p. 68.

same kind of natural classification which is best in biology will apply also in mineralogy, in chemistry, or in astronomy. The universal logical principles which underlie all classifications are of course the same in natural history as in the sciences of brute matter, but the special logical resemblances which arise from the relation of parent and offspring will not be found to prevail between different kinds of crystals or mineral bodies.

The genealogical view of the mutual relations of animals and plants leads us to discard all notions of any regular progression of living forms, or any theory as to their symmetrical relations. It was at one time a great question whether the ultimate scheme of natural classification would prove to be in a simple line, or a circle, or a combination of circles. Macleay's once celebrated system was a circular one, and each class-circle was composed of five order-circles, each of which was composed again of five tribe-circles, and so on, the subdivision being at each step into five minor circles. Thus he held that in the animal kingdom there were five sub-kingdoms—the Vertebrata, Annulosa, Radiata, Acrita, and Mollusca. Each of these was again divided into five—the Vertebrata consisting of Mammalia, Reptilia, Pisces, Amphibia, and Aves[g]. It is quite evident that in any such symmetrical system the animals were made to suit themselves to the classes instead of the classes being suited to the animals.

We now perceive that the ultimate system will be an almost infinitely extended genealogical tree, which will be capable of representation by lines on a plane surface of sufficient extent. But there is not the least reason to suppose that this tree will have a symmetrical form. Some branches of it would be immensely developed compared with others. In some cases a form may have pro-

[g] Swainson, 'Treatise on the Geography and Classification of Animals,' 'Cabinet Cyclopædia,' p. 201.

pagated itself almost from primeval times with little variation.

In other cases frequent differentiations will have occurred. Strictly speaking, this genealogical tree ought to represent the descent of each individual living form now existing or which has existed. It should be as personal and minute in its detail of relations, as the Stemma of the Kings of England. We must not assume that any two forms are absolutely and exactly alike, and in any case they are numerically distinct. Every parent then must be represented at the apex of a series of divergent lines, representing the generation of so many children. Any complete and perfect system of classification must regard individuals as the infimæ species. But as in the lower races of animals and plants the differences between individuals are usually very slight, and apparently unimportant, while the numbers of such individuals are immensely great, beyond all possibility of separate treatment, scientific men have always stopped at some convenient but arbitrary point, and have assumed that forms so closely resembling each other as to present no constant difference were all of one kind. They have, in short, fixed their attention entirely upon the main features of family difference. In the genealogical tree which they have been unconsciously aiming to construct, diverging lines meant races diverging in character, and the purpose of all efforts at so-called natural classification was to trace out the relationships between existing plants or animals. Now it is evident that hereditary descent may have in different cases produced very different results as regards the problem of classification. In some cases the differentiation of characters may have been very frequent, and specimens of all the characters produced may have been transmitted to the present time. A living form will then have, as it were, an almost infinite number of cousins of various degrees,

and there will be an immense number of forms finely graduated in their resemblances. Exact and distinct classification will then be almost impossible, and the wisest course will be not to attempt arbitrarily to distinguish forms closely related in nature, but to allow that there exist transitional forms of every degree, to mark out if possible the extreme limits of the family relationship, and perhaps to select the most generalized form, or that which presents the greatest number of close resemblances to others of the family, as the *type* of the whole.

Mr. Darwin, in his most interesting work upon Orchids, points out that the tribe of Malaxeæ are distinguished from Epidendreæ by the absence of a caudicle to the pollinia, but as some of the Malaxeæ have a minute caudicle the division really breaks down in the most essential point.

'This is a misfortune,' he remarks[h], 'which every naturalist encounters in attempting to classify a largely developed or so-called natural group, in which, relatively to other groups, there has been little extinction. In order that the naturalist may be enabled to give precise and clear definitions of his divisions, whole ranks of intermediate or gradational forms must have been utterly swept away: if here and there a member of the intermediate ranks has escaped annihilation, it puts an effectual bar to any absolutely distinct definition.'

In other cases a particular plant or animal may perhaps have transmitted its form from generation to generation almost unchanged, or, what comes to the same result, those forms which diverged in character from the parent stock, may have proved unsuitable to their circumstances, and may have perished sooner or later. We shall then find a particular form standing apart from all others, and marked by various distinct characters. Occasionally we

[h] Darwin, 'Fertilization of Orchids,' p. 159.

may meet with specimens of a race which was formerly far more common but is now undergoing extinction, and is nearly the last of its kind. Thus we may explain the occurrence of exceptional forms such as are found in the Amphioxus. The Equisetaceæ perplex botanists by their want of affinity to other orders of Acrogenous plants. This doubtless indicates that their genealogical connexion with other plants must be sought for in the most distant past ages of geological development.

Constancy of character, as Mr. Darwin has said[i], is what is chiefly valued and sought after by naturalists; that is to say naturalists wish to find some distinct family-mark, or group of characters by which they may clearly recognise the relationship of descent between a large group of living forms. It is accordingly a great relief to the mind of the naturalist when he comes upon a definitely marked group, such as the Diatomaceæ, which are clearly separated from their nearest neighbours the Desmidiaceæ by their siliceous framework and the absence of chlorophyll. But we must no longer think that because we fail in detecting constancy of character the fault is in our classificatory sciences. Where gradation of character really exists, we must devote ourselves to defining and registering the degrees and limits of that gradation. The ultimate natural arrangement will often be devoid of strong lines of demarcation.

Let naturalists, too, form their systems of natural classification with all care they can, yet it will certainly happen from time to time that new and exceptional forms of animals or vegetables will be discovered, and will require the modification of the system. A natural system is directed, as we have seen, to the discovery of empirical laws of correlation, but these laws being purely empirical will frequently be falsified by more extensive investiga-

[i] 'Descent of Man,' vol. i. p. 214.

tion. From time to time the notions of naturalists have been greatly widened, especially in the case of Australian animals and plants, by the discovery of unexpected combinations of organs, and such events must often happen in the future. If indeed the time shall come when all the forms of plants are discovered and accurately described, the science of Systematic Botany will then be placed in a new and more favourable position, as remarked by Alphonse Decandolle[k].

It ought, I think, to be allowed that though the genealogical classification of plants or animals is doubtless the most natural and instructive of all, it is not necessarily the best for all purposes. There may be correlations of properties important for medicinal, or other practical purposes, which do not correspond to the correlations of descent. We must regard the bamboo as a tree rather than a grass, although it is botanically a grass. For legal purposes we may still with advantage continue to treat as fish, the whale, seal, and other cetaceæ. We must class plants together according as they are Arctic, or Alpine, or belong to the temperate, sub-tropical or tropical regions. There may be some causes of likeness apart from hereditary relationship, and in a logical and practical point of view we must not attribute exclusive excellence to any one method of classification.

Classification by Types.

Perplexed by the difficulties arising in natural history from the discovery of intermediate forms, naturalists have resorted to what they call classification by types. Instead of forming one distinct class defined by the invariable possession of certain assigned properties, and rigidly including or excluding objects according as they do or

[k] 'Laws of Botanical Nomenclature,' p. 16.

do not possess all these properties, naturalists select a typical form or specimen, and they group around it all other forms or specimens which resemble this type more than any other selected type. 'The type of each genus,' we are told[1], 'should be that species in which the characters of its group are best exhibited and most evenly balanced.' It would usually consist of those descendants of a form which had undergone little alteration, while other descendants had suffered slight differentiation in various directions.

It would be a great mistake to suppose that this classification by types is a logically distinct method. It is either not a real method of classification at all, or it is a merely abbreviated mode of representing a very complicated system of arrangement. A class must be defined by the invariable presence of certain common properties. If, then, we venture to include an individual in which one of these properties does not appear, we either fall into logical contradiction, or else we form a new class with a new definition. Even a single exception constitutes a new class by itself, and by calling it an exception we merely imply that this new class closely resembles that from which it diverges in one or two points only. Thus if in the definition of the natural order of Rosaceæ, we find that the seeds are one or two in each carpel, but that in the genus Spiræa there are three or four, this must mean either that the number of seeds is not a part of the fixed definition of the class, or else that Spiræa does not belong to that class, though it may be closely approximated to it. Naturalists continually find themselves between two horns of a dilemma; if they restrict the number of marks specified in a definition so that every form intended to come within the class shall possess all

[1] Waterhouse, quoted by Woodward in his 'Rudimentary Treatise of Recent and Fossil Shells,' p. 61.

those marks, it will then be usually found to include too many forms; if the definition be made more particular, the result is to produce so-called anomalous genera, which, while they are held to belong to the class, do not in all respects conform to its definition. The practice has hence arisen of allowing considerable latitude in the definition of natural orders. The family of Cruciferæ, for instance, forms an exceedingly well marked natural order, and among its characters we find it specified that the fruit is a pod, divided into two cells by a thin partition, from which the valves generally separate at maturity; but we are also informed that, in a few genera, the pod is one-celled, or indehiscent, or separates transversely into several joints[m]. Now this must either mean that the formation of the pod is not an essential point in the definition, or that there are several closely associated families.

The same holds true of typical classification. The type itself is an individual, not a class, and no other object can be exactly like the type. But so soon as we abstract the individual peculiarities of the type and thus specify a finite number of qualities in which other objects may resemble the type, we immediately constitute a class. If some objects resemble the type in some points and others in other points, then each definite collection of points of resemblance constitutes intensively a separate class. The very notion of classification by types is in fact erroneous in a strictly logical point of view. The naturalist is constantly occupied by endeavouring to mark out definite groups of living forms, where the forms themselves do not in many cases admit of any such rigorous lines of demarcation. A certain laxity of logical method is thus apt to creep in, the only remedy for which will be

[m] Bentham's 'Handbook of the British Flora' (1866), p. 25.

the frank recognition of the fact that according to the theory of hereditary descent, the gradation of characters is probably the rule, and the precise demarcation between groups the exception.

Natural Genera and Species.

One important result of the establishment of the theory of evolution, is to explode all notions about the existence of natural groups constituting separate creations. Naturalists have long held that every plant belongs to some species or group, marked out by invariable characters, which do not change by difference of soil, climate, cross-breeding, or other circumstances. They were unable to deny the existence of such things as sub-species, varieties, or hybrids, so that a species of plants was often sub-divided and classified within itself. But then the differences upon which this sub-classification depended were supposed to be variable, and thus distinguished from the invariable characters imposed upon the whole species at its creation. Similarly a Natural Genus was a group of species, and was marked out from other genera by eternal differences of still greater importance.

We now, however, perceive that the existence of any such groups as genera and species is an arbitrary creation of the naturalist's mind. All resemblances of plants, indeed, are natural, so far as they express their hereditary affinities, but this applies as well to the variations within the species as to the species itself, or the larger natural classes. All is a matter of degree. The deeper differences between plants have been produced by the differentiating action of circumstances during millions of years, so that it would naturally require millions of years to undo this result, and prove experimentally that the forms can be approximated together again. Sub-species may often have

arisen within historical times, and varieties approaching to sub-species may often be produced by the horticulturist in a few years. Such varieties can easily be brought back to their original form, or, if placed in the original circumstances, will themselves revert to that form; but according to Darwin's views all forms are capable of unlimited change, and, it might possibly be, unlimited reversion, if sufficient time and suitable circumstances be granted.

Many fruitless and erroneous attempts have been made to establish some rigorous criterion of specific and generic difference, so that these classes might have a definite value or rank in all branches of biology. Linnæus adopted the view that the species was to be defined as a distinct Creation saying[n], 'Species tot numeramus, quot diversæ formæ in principio sunt creatæ,' or again, 'Species tot sunt, quot diversas formas ab initio produxit Infinitum Ens; quæ formæ, secundum generationis inditas leges, produxere plures, at sibi semper similes.' Of genera he also says[o], 'Genus omne est naturale, in primordio tale creatum.' It was a common doctrine added to and essential to that of distinct creation that these species could not produce intermediate and variable forms, so that we find Linnæus in another work obliged by the ascertained existence of hybrids to take a different view; he says[p], 'Novas species immo et genera ex copula diversarum specierum in regno vegetabilium oriri primo intuitu paradoxum videtur; interim observationes sic fieri non ita dissuadent.' Even supposing in the present day that we could assent to the notion of a certain number of distinct creational acts, this notion would not help us in the theory of classi-

[n] 'Philosophia Botanica' (1770), § 157, p. 99.

[o] Ibid, § 159, p. 100.

[p] 'Amœnitates Academicæ' (1744), vol. i. p. 70. Quoted in 'Edinburgh Review,' October 1868, vol. cxxviii. pp. 416, 417.

fication. Naturalists have never pointed out any separate method of deciding what are the results of distinct creations, and what are not. As Darwin says[q], 'the definition must not include an element which cannot possibly be ascertained, such as an act of creation.' It is, in fact, by investigation of forms and classification that we should ascertain what were distinct creations and what were not; this information would be a result and not a means of classification.

The eminent naturalist Agassiz seems to consider that he has discovered an important principle, to the effect that general plan or structure is the true ground for the discrimination of the great classes of animals, which may be called branches of the animal kingdom[r]. He also thinks that genera are definite and natural groups. 'Genera,' he says[s], 'are most closely allied groups of animals, differing neither in form, nor in complication of structure, but simply in the ultimate structural peculiarities of some of their parts; and this is, I believe, the best definition which can be given of genera.' But it is surely apparent that there are endless degrees both of structural peculiarity and of complication of structure. It is impossible to define the amount of structural peculiarity which constitutes the genus as distinguished from the species.

The form which any classification of plants or animals tends to take is that of an unlimited series of subaltern classes. Originally botanists confined themselves for the most part to a limited number of such classes; thus Linnæus adopted Class, Order, Genus, Species, and Variety, and even seemed to think that there was something essentially natural in a five-fold arrangement of groups[t].

q 'Descent of Man,' vol. i. p. 228.

r Agassiz, 'Essay on Classification,' p. 219.

s Ibid. p. 249.

t 'Philosophia Botanica,' § 155, p. 98.

With the progress of botany intermediate and additional divisions have gradually been introduced. According to the Laws of Botanical Nomenclature adopted by the International Botanical Congress, held at Paris[u] in August, 1867, no less than twenty-one names of classes are recognised—namely, Kingdom, Division, Sub-division, Class, Sub-class, Cohort, Sub-cohort, Order, Sub-order, Tribe, Sub-tribe, Genus, Sub-genus, Section, Sub-section, Species, Sub-species, Variety, Sub-variety, Variation, Sub-variation. It is allowed by the authors of this scheme, that the definition or degree of importance to be attributed to any of these terms may vary in a certain degree according to individual opinion. The only point on which botanists are not allowed discretion is as to the order of the successive sub-divisions; the division of genera into tribes, or of tribes into orders; any inversion, in short, of the arrangement being inadmissible. There is no reason to suppose that even the above list is complete and inextensible. The Botanical Congress itself recognised the distinction between variations according as they are Seedlings, Half-breeds, or *Lusus Naturæ*. The complication of the inferior classes is increased again by the existence of *hybrids*, arising from the fertilization of one species by another deemed a distinct species, nor can we place any limit to the minuteness of discrimination of degrees of breeding short of an actual pedigree of descent.

It will be evident to the reader that in the remarks upon classification as applied to the Natural Sciences, given in this and the preceding sections, I have not in the least attempted to treat the subject in a manner adequate to its extent and importance. A volume would be insufficient for tracing out the principles of scientific method

[u] 'Laws of Botanical Nomenclature,' by Alphonse Decandolle, translated from the French, 1868, p. 19.

specially applicable to these branches of science. What more I may be able to say upon the subject will be better said, if ever, when I am able to take up the closely-connected subject of Scientific Nomenclature, Terminology, and Descriptive Representation. In the meantime, I have wished to show, in a negative point of view, that natural classification in the animal and vegetable kingdoms is a special problem, and that the special methods and difficulties to which it gives rise are not those common to all cases of classification, as so many physicists have supposed. Genealogical resemblances are only a special case of resemblances in general.

Unique or Exceptional Objects.

In framing a system of classification in almost any branch of science, we must expect to meet with unique or peculiar objects, which are so called because they seem to stand alone, having few analogies with other objects. They may also be said to be *sui generis*, each unique object forming, as it were, a class by itself; or they are called *nondescript*, because in thus standing apart it is difficult to find terms in which to explain their properties. The rings of Saturn, for instance, form a unique object among the celestial bodies. We have indeed considered this and many other instances of unique objects in the preceding chapter, on Exceptional Phenomena. Apparent, Singular, and Divergent Exceptions especially, are analogous in nature to unique objects.

In the classification of the elements, Carbon stands apart as a substance entirely unique in its powers of producing compounds. It is considered to be a quadrivalent element, and it obeys all the ordinary laws of chemical combination. Yet it manifests powers of affinity in such an exalted degree that the substances in which it

appears are more numerous than all the other compounds known to chemists. Almost the whole of the substances which have been called organic contain carbon, and are probably held together by the carbon atoms, so that many chemists are now inclined to abandon the name Organic Chemistry, and substitute the name Chemistry of the Carbon Compounds. It used to be believed that the production of the so-called organic compounds was due solely to the action of a vital force, or some inexplicable cause involved in the phenomena of life, but it is now found that chemists are able to commence with the elementary materials, pure carbon, hydrogen, and oxygen, and by strictly chemical operations, combine these together so as to form complicated organic compounds. So many compounds have already been thus formed that the probability is very great that many others will be so formed in the course of time, and we might be inclined to generalize, and infer that all so-called organic compounds might ultimately be produced without the agency of living beings. Thus the distinction between the organic and the inorganic kingdoms seems to be breaking down, but our wonder at the peculiar powers of carbon must increase at the same time.

In considering generalization, the law of continuity was applied chiefly to physical properties capable of mathematical treatment. But in the classificatory sciences, also, the same important principle is often beautifully exemplified. Many objects or events seem to be entirely exceptional and abnormal, and in regard to degree or magnitude they may be so termed. We might adduce examples on the one hand of such extreme cases, but it is often easy to show, on the other hand, that they are connected by intermediate links with other apparently different cases.

In the organic kingdoms of nature there is a common

groundwork of similarity running through all classes, but particular actions and processes present themselves conspicuously in particular families and classes. Tenacity of life is most marked in the Rotifera, and some other kinds of microscopic organisms, which can be dried and boiled without loss of life. Reptiles are distinguished by torpidity, and the length of time they can live without food. Birds, on the contrary, exhibit ceaseless activity and high muscular power. The ant is as conspicuous for intelligence and size of brain among insects as the quadrumana and man among vertebrata. Among plants the Leguminosæ are distinguished by a tendency to sleep, folding their leaves at the approach of night. In the genus Mimosa, especially the Mimosa pudica, commonly called the sensitive plant, the same tendency is magnified into an extreme irritability, almost resembling voluntary motion. More or less of the same irritability probably belongs to vegetable forms of every kind, but it is of course to be investigated with special ease in such an extreme case. In the Gymnotus and Torpedo, we find that organic structures can act like galvanic batteries. Are we to suppose that such animals are entirely anomalous exceptions; or may we not justly expect to find less intense manifestations of electric action in all animals and plants?

In the animal world we find many phenomena which seem to be peculiar to certain classes, but are afterwards found to differ but in degree from what is always present. The lower animals, for instance, seem to differ entirely from the higher ones in the power of reproducing lost limbs. A kind of crab has the habit of casting portions of its claws when much frightened, but they soon grow again. There are multitudes of smaller animals which, like the Hydra, may be cut in two and yet live and develop into new complete individuals. No mammalian animal can repro-

duce a limb, and in appearance there is no analogy. But it was suggested by Blumenbach that the healing of a wound in the higher animals really represents in a lower degree the power of reproducing a limb. That this is true may be shown by adducing a multitude of intermediate cases, each adjoining pair of which are clearly analogous, so that we pass gradually from one extreme to the other. Darwin holds, moreover, that any such restoration of parts is closely connected with that perpetual replacement of the particles which causes every organized body to be after a time entirely new as regards its constituent substance. In short, we approach to a great generalization under which all the phenomena of growth, restoration, and maintenance of organs are effects of one and the same power[x]. It is perhaps still more surprising to find that the complicated process of sexual reproduction in the higher animals may be gradually traced down to a simpler and simpler form, which at last becomes undistinguishable from the budding out of one plant from the stem of another. By a great generalization we may regard all the modes of reproduction of organic life as alike in their nature, and varying only in complexity of development[y].

Limits of Classification.

Science can extend only so far as the power of accurate classification extends. If we cannot detect resemblances, and assign their exact character and amount, we cannot have that generalized knowledge which constitutes science; we cannot infer from case to case. It will readily be

[x] Darwin, 'The Variation of Animals and Plants,' vol. ii. pp. 293, 359, &c.; quoting Paget, 'Lectures on Pathology,' 1853, pp. 152, 164.

[y] Ibid. vol. ii. p. 372.

observed that classification is the opposite process to discrimination. If we feel that two tastes differ, for instance, the tastes of two specimens of wine, the mere fact of difference existing prevents inference. The detection of the difference saves us, indeed, from false inference, because so far as difference exists, all inference is impossible. But classification consists in detecting resemblances of all degrees of generality, and ascertaining exactly how far such resemblances extend, while assigning precisely at the same time the points at which difference begins. It enables us, then, at once to generalize and make inferences where it is possible, and it saves us at the same time from going too far. Full classifications constitute a complete record of all our knowledge of the objects or events classified, and the limits of exact or scientific knowledge are identical with the limits of classification.

It must by no means be supposed that every group of natural objects will be found capable of rigorous classification. There may be substances which vary by insensible degrees, consisting, for instance, in varying mixtures of simpler substances. Granite is a mixture of quartz, felspar, and mica, but there are hardly two specimens in which the proportions of these three constituents are alike, and it would be impossible to lay down definitions of distinct species of granite without finding an infinite variety of intermediate species. The only true classification of granites, then, would be founded on the proportions of the constituents present, and a chemical or microscopic analysis would be requisite, in order that we should assign any specimen to its true position in the series. Granites vary, again, by insensible degrees, as regards the magnitude of the crystals of felspar and mica. Precisely similar remarks might be made concerning the classification of other plutonic rocks, such as syenite, basalt, pumice-stone, lava, tuff, &c.

The nature of a ray of homogeneous light is strictly defined, either by its place in the spectrum or by the corresponding wave-length, but a ray of mixed light admits of no simple classification; any of the infinitely numerous rays of the continuous spectrum may be present or absent, or present in various intensities, so that we can only class and define a mixed colour by defining the intensity and wave-length of each ray of homogeneous light which is present in it. Complete spectroscopic analysis and the determination of the intensity of every part of the spectrum yielded by a mixed ray is requisite for its accurate classification. Nearly the same may be said of complex sounds. A simple sound undulation, if we could meet with such a sound, would admit of precise and exhaustive classification as regards pitch, the length of wave, or the number of waves reaching the ear per second being a sufficient criterion. But almost all ordinary sounds, even those of musical instruments, consist of complex aggregates of undulations of several different pitches, and in order to classify the sound we should have to measure the intensities of each of the constituent sounds, a work which has been partially accomplished by Professor Helmholtz, as regards the vowel sounds. The different tones of voice distinctive of different individuals must also be due to the intermixture of minute waves of various pitch, which are at present quite beyond the range of experimental investigation. We cannot, then, at present, attempt to classify the different kinds or *timbres* of sound.

The difficulties of classification are even greater when a varying phenomenon cannot be shown to be a mixture of simpler phenomena. If we attempt, for instance, to classify the tastes of natural and artificial substances, we may rudely group them according as they are sweet, bitter, saline, alkaline, acid, astringent, or fiery; but it is evident that these groups are bounded by no sharp lines

of definition. Tastes of mixed or intermediate character may exist almost *ad infinitum*, and, what is still more troublesome, the tastes clearly united within one class may differ more or less from each other, without our being able to arrange them in subordinate genera and species. The same remarks may be made concerning the classification of odours, which may be roughly grouped according to the arrangement of Linnæus as, Aromatic, Fragrant, Ambrosiac, Alliaceous, Fetid, Virulent, Nauseous. Within each of these vague classes, however, there would be infinite shades of variety, and each class would graduate probably into each other class. The varieties of odour which can be discriminated by an acute olfactory organ are almost infinite; every rock, stone, plant, or animal has some slight odour, and it is well known that dogs, or even blind human beings, can discriminate persons by a slight distinctive odour which usually passes unnoticed.

Nearly similar remarks may be made concerning the higher feelings of the human mind, usually called emotions. We know what is anger, grief, fear, hatred, love; and many systems for classifying these feelings have been proposed at one time or another. They may be roughly distinguished according as they are pleasurable or painful, prospective or retrospective, selfish or sympathetic, active or passive, and possibly in many other ways, but each mode of arrangement will be indefinite and unsatisfactory when followed into details. As a general rule, the emotional state of the mind at any moment will be neither pure anger nor pure fear, nor any one pure feeling, but an indefinite and complex aggregate of feelings. It may be that the state of mind is really a sum of several distinct modes of agitation, just as a mixed colour is the sum of the several distinct rays of the spectrum. In this case there may be more hope of some method of analysis being successfully applied at some future time. But it may

be found that states of mind really graduate into each other, so that rigorous classification would prove to be hopeless.

A little reflection will show that there are whole worlds of existences which in like manner are incapable of logical analysis and classification. One friend may be able to single out and identify another friend by his countenance among a million other countenances. Faces are capable of infinite discrimination, but who shall classify and define them, or say by what particular shades of feature he does judge. There are of course certain distinct types of face, but each type is connected with each other type by infinite intermediate specimens. We may classify melodies according to the major or minor key, the character of the time, and some other distinct points; but every melody has independently of such circumstances its own distinctive character and effect upon the mind. Similar remarks might be made concerning a multitude of other circumstances. We can detect differences between the styles of literary, musical, or artistic compositions. We can even in some cases assign a picture to its painter, or a symphony to its composer, by a subtle feeling of resemblances or differences of character and expression, which may be felt, but cannot be described.

Finally, it is apparent that in human character there is unfathomable and inexhaustible diversity. Every mind is more or less like every other mind; there is always a basis of similarity, but there is a superstructure of feelings, impulses, or motives which is distinctive for each person. We can often, indeed, predict the general character of the feelings or actions which will be produced in a given individual well known to us, by a given external event, but we also know that we are often inexplicably at fault in all our inferences. No one can safely generalize upon the subtle variations of temper and emotion which may

arise even in a person of ordinary character. As human knowledge and civilization progress, these characteristic differences tend to develop and multiply themselves rather than decrease. Character grows more evidently many-sided. Two well educated Englishmen are far better distinguished from each other than two common labourers, and these are better distinguished, again, than two Australian aborigines. Thus the complexities of existing phenomena develop themselves more rapidly than scientific method can overtake them. In spite of all the boasted powers of science, we cannot really apply method to those existences, namely, our own minds and characters, which are more important to us than all the stars and nebulæ.

BOOK VI.

CHAPTER XXXI.

REFLECTIONS ON THE RESULTS AND LIMITS OF SCIENTIFIC METHOD.

BEFORE concluding a work on the Principles of Science, it will not be inappropriate to add some remarks upon the limits and ultimate bearings of the knowledge which we may acquire by the constant employment of scientific method. All science consists, it has several times been stated, in the detection of identities and uniformities in the action of natural agents. The purpose of inductive inquiry is to ascertain the apparent existence of necessary connexion between causes and effects, the establishment of natural laws. Now so far as we thus learn the invariable course of nature, the future becomes the necessary sequel of the present, and we are brought beneath the sway of powers with which nothing can interfere.

By degrees it is found, too, that the chemistry of organized substances is not widely separated from, but is rather continuous with, that of earth and stones. Life itself seems to be nothing but a special form of that energy which is manifested in heat and electricity and mechanical force. The time may come, it almost seems,

when the tender mechanism of the brain will be traced out, and every thought reduced to the expenditure of a determinate weight of nitrogen and phosphorus. No apparent limit exists to the success of scientific method in weighing and measuring, and reducing beneath the sway of law, the phenomena both of matter and of mind. And if mental phenomena be thus capable of treatment by the balance and the micrometer, can we any longer hold that mind is distinct from matter? Must not the same inexorable reign of law, which is apparent in the motions of brute matter, be extended to the most subtle feelings of the human heart? Are not plants and animals and ultimately man himself, merely crystals, as it were, of a complicated form? If so, our boasted Free Will becomes a delusion, Moral Responsibility a fiction, Spirit a mere name for the more curious manifestations of material energy. All that happens, whether right or wrong, pleasurable or painful, is but the outcome of the necessary relations of time and space and force, and of the laws of matter emerging from them, which are fixed in the very nature of things.

Materialism seems, then, to be the coming religion, and resignation to the nonenity of human will the only duty. Such may not generally be the reflections of men of science, but I believe that we may thus describe the secret feelings of fear which the constant advance of scientific investigation excites in the minds of many who view it from a distance. Is science, then, essentially atheistic and materialistic in its tendency? Does the uniform action of material causes, which we learn with an ever increasing approach to certainty, preclude the hypothesis of an intelligent and benevolent Creator, who has not only designed the existing universe, but who still retains the power to alter its course from time to time?

To enter actually upon theological discussions would be evidently beyond the scope of this work. It is with the scientific method common to all the sciences, and not with any of the separate sciences, that we are concerned. Theology therefore would be at least as much beyond my scope as chemistry or geology. But I believe that grave misapprehensions exist as regards the very nature of this scientific method. There are scientific men who assert that the interposition of Providence is impossible, and prayer an absurdity, because the laws of nature are inductively proved to be invariable. Inferences are drawn not so much from particular sciences as from the logical foundations of science itself, to negative the impulses and hopes of men. Now I may properly venture to state that my own studies in logic lead me to call in question all such negative inferences. Those so-called laws of nature are uniformities observed to exist in the action of certain material agents, but it is logically impossible to show that all other agents must behave as these do. The too exclusive study of particular branches of physical science seems in some cases to generate an over confident and dogmatic spirit. Rejoicing in the success with which a few groups of facts are brought beneath the apparent sway of laws, the investigator hastily assumes that he is close upon the ultimate springs of being. A particle of gelatinous matter is found to obey the ordinary laws of chemistry; yet it moves and lives. The world is therefore asked to believe that chemistry can resolve the mysteries of existence.

The Meaning of Natural Law.

Pindar speaks of Law as the Ruler of the Mortals and the Immortals, and it seems to be commonly supposed that the so-called Laws of Nature, in like manner, rule

man and his Creator. The course of nature is regarded as being determined by invariable principles of mechanics which have acted since the world began, and will act for infinite ages to come. Even if the origin of all things be attributed to an intelligent creative mind, that Being is regarded as having yielded up arbitrary power, and as being subject like a human legislator to the laws which he has himself enacted. Such notions I should describe as superficial and erroneous, being derived, as I think, from false views of the nature of scientific inference, and the degree of certainty of the knowledge which we acquire by inductive investigation.

A law of nature, as I regard the meaning of the expression, is not a uniformity which must be obeyed by all objects, but merely a uniformity which is as a matter of fact obeyed by those objects which have come beneath our observation. There is nothing whatever incompatible with logic in the discovery of objects which should prove exceptions to any law of nature. Perhaps the best established law is that which asserts an invariable correlation to exist between gravity and inertia, so that all gravitating bodies are found to possess inertia, and all bodies possessing inertia are found to gravitate. But it would be no reproach to our scientific method, if something were ultimately discovered to possess gravity without inertia. Strictly defined and correctly interpreted, the law itself would acknowledge the possibility; for with the statement of every law we ought properly to join an estimate of the number of instances in which it has been observed to hold true, and the probability thence calculated, that it will hold true in the next case. Now as we before found (vol. i. p. 299) no finite number of instances can warrant us in expecting with certainty that the next instance will be of like nature; in the formulas yielded by the inverse method of probabilities a unit always

appears to represent the probability that our inference will be mistaken. I demur to the assumption that there is any necessary truth even in such fundamental laws of nature as the Indestructibility of Matter, the Conservation of Force, or the Laws of Motion. Certain it is that men of science have recognised the conceivability of other laws, or even investigated their mathematical conditions. Sir George Airy investigated the mathematical conditions of a perpetual motion (vol. i. p. 256), and Laplace and Newton discussed various imaginary laws of forces inconsistent with those so far observed to operate in the universe (vol. ii. pp. 304, 392).

The laws of nature, as I venture to regard them, are simply general propositions concerning the correlation of properties which have been observed to hold true of bodies hitherto observed. On the assumption that our experience is of adequate extent, and that no arbitrary interference takes place, we are then able to assign the probability, always less than certainty, that the next object of the same apparent nature will conform to the same law.

Infiniteness of the Universe.

We may safely accept as a satisfactory scientific hypothesis the doctrine so grandly put forth by Laplace, who asserted that a perfect knowledge of the universe, as it existed at any given moment, would give a perfect knowledge of what was to happen thenceforth and for ever after. Scientific inference is impossible, unless we may regard the present as the necessary outcome of what is past, and the necessary cause of what is to come. To the view of Perfect Intelligence nothing is uncertain. The astronomer can calculate the positions of the heavenly bodies when thousands of generations of men shall have

passed away, and in this fact we have some illustration, as Laplace remarks, of the power which scientific prescience may attain. Doubtless, too, all efforts in the investigation of nature tend to bring us nearer to the possession of that ideally perfect power of intelligence. Nevertheless, as Laplace with profound wisdom adds[a], we must ever remain at an infinite distance from the goal of our aspirations.

Let us assume, for a time at least, as a highly probable hypothesis, that whatever is to happen must be the outcome of what is; there then arises the question, What is? Now our knowledge of what exists must ever remain imperfect and fallible in two respects. Firstly, we do not know all the matter that has been created, nor the exact manner in which it has been distributed through space. Secondly, assuming that we had that knowledge, we should still be wanting in a perfect knowledge of the way in which the particles of matter will act upon each other. The power of scientific prediction extends at the most to the limits of the data employed. Every conclusion is purely hypothetical and conditional upon the non-interference of agencies previously undetected. The law of gravity asserts that every body tends to approach towards every other body, with a certain determinate force, but even supposing the law to hold true, it does not assert that the body *will* approach. No single law nor science can warrant us in making any one absolute prediction. We must know all the laws of nature and all the existing agents acting according to those laws before we can say what will occur. To assume, then, that scientific method can take everything within its cold embrace of uniformity, is to imply that the Creator cannot outstrip the intelligence of his creatures, and that the existing

a 'Théorie Analytique des Probabilités,' quoted by Babbage, 'Ninth Bridgwater Treatise,' p. 173.

universe is not infinite in extent and complexity, an assumption for which I can see no logical basis whatever.

The Indeterminate Problem of Creation.

A second and very serious misapprehension concerning the import of a law of nature may now be pointed out. It is not uncommonly supposed that a law determines the character of the results which shall take place, as, for instance, that the law of gravity determines what force of gravity shall act upon a given particle. Surely a little reflection must render it plain that a law by itself determines nothing. It is a law *plus* agents obeying that law which have results, and it is no part of the law to govern or define the number and place of its own agents. Whether a particle of matter shall gravitate, depends not upon the law of Newton only, but upon the distribution of surrounding particles. The theory of gravitation may perhaps be true throughout all time and in all parts of space, and even the Creator may never find occasion to create those possible exceptions to it which I have asserted to be conceivable. Let this be as it may, and our science cannot certainly determine the question, yet the theory of gravitation itself gives no indication of the forces which may be brought to act at any point of space. The force of gravitation acting upon any particle depends, as we have seen, upon the number, mass, distance, and relative position of all the other particles of matter within the bounds of space at the instant in question. Even assuming that all matter when once distributed through space at the Creation, was thenceforth to act in an invariable manner without subsequent interference, yet the actual configuration of matter at any moment, and

the consequent results of the law of gravitation must have been entirely a matter of free choice.

Chalmers has most distinctly pointed out that the existing *collocations* of the material world are at least as important as the laws which the objects obey. He remarks that a certain class of writers entirely overlook the distinction, and forget that mere laws without collocations would have afforded no security against a turbid and disorderly chaos[b]. Mr. J. S. Mill has recognised[c] the truth of Chalmers' statement, without drawing the proper inferences from it. He says[d] of the distribution of matter through space, 'We can discover nothing regular in the distribution itself; we can reduce it to no uniformity, to no law.' More lately the Duke of Argyle in his well known work on the 'Reign of Law' has drawn attention to the profound distinction between laws and collocations of causes.

The original conformation of the material universe was, so far as we can possibly tell, free from all restriction. There was unlimited space in which to frame it, and an unlimited number of material particles, each of which could be placed in any one of an infinite number of different positions. It must also be added that each particle might be endowed with any one of an infinite number of degrees of *vis viva* acting in any one of an infinitely infinite number of different directions. The problem of Creation was, then, what a mathematician would call an indeterminate problem, and it was indeterminate in an infinitely infinite number of ways. Infinitely numerous and various universes might then have been fashioned by the various distribution of the

b 'First Bridgwater Treatise' (1834), pp. 16–24.

c 'System of Logic,' 5th edit. bk. III. chap. V. § 7. Chap. XVI. § 3.

d Ibid. vol. i. p. 384.

original nebulous matter, although all the particles of matter should obey the one law of gravity.

Lucretius tells us how in the original rain of atoms some of these little bodies diverged from the rectilineal direction, and coming into contact with other atoms gave rise to the various combinations of substances and phenomena which exist. He omitted, indeed, to tell us whence the atoms came, or by what force some of them were caused to diverge, but surely these omissions involve the whole question. I accept the Lucretian conception of creation when properly supplemented. Every atom which existed in any point of space must have existed there previously, or must have been created there by a previously existing Power. When placed there it must have had a definite mass and a definite energy, kinetic or potential as regards other existing atoms. Now, as before remarked, an unlimited number of atoms can be placed in unlimited space in an entirely unlimited number of modes of distribution. Out of infinitely infinite choices which were open to the Creator, that one choice must have been made which has yielded the universe as it now exists.

It would indeed be a mistake to suppose that the law of gravity, when it holds true, is no restriction in the distribution of force. That law is a geometrical law, and it would in many cases be mathematically impossible, as far as we can see, that the force of gravity acting on one particle should be small while that on a neighbouring particle was great. We cannot conceive that even Omnipotent Power should make the angles of a triangle less or greater than two right angles. The primary laws of thought and the fundamental notions of the mathematical sciences do not seem to us to admit of any alteration. Into the metaphysical origin and meaning of the apparent necessity attaching to such laws I have not

attempted to inquire in this work, and it is not requisite for my present purpose. If the law of gravity were the only law of nature and the Creator had chosen to render all matter obedient to that law, there would doubtless be restrictions upon the effects derivable from any one distribution of matter.

Hierarchy of Natural Laws.

A further consideration inevitably presents itself. A natural law like that of gravitation expresses a certain uniformity in the mode of action of agents submitted to it, and this uniformity produces, as we have seen, certain geometrical restrictions upon the effects which those agents may produce. But there are other forces and laws besides those of gravity. One force may override another, and two laws may each be obeyed and may each disguise the action of the other. In the intimate constitution of matter there may be hidden springs of force which, while acting in accordance with their own fixed laws, may lead to sudden and unexpected changes. So at least it has been found from time to time in the past, and so there is every reason to believe it will be found in the future. To the ancients it seemed incredible that one lifeless stone could make another leap towards it. A piece of iron while it obeys the magnetic forces of the loadstone does not the less obey the law of gravity. A plant also gravitates downwards as regards every constituent cell or fibre, and yet it persists in growing upwards. Life altogether is an exception to the simple phenomena of mineral substances, not in the sense of disproving those laws, but in that of superadding forces of new and inexplicable character. Doubtless no law of chemistry is broken by the action of the nervous cells, and no law of physics by the pulses of the nervous

fibres, but something requires to be added to our sciences in order that we even explain these subtle phenomena.

Now there is absolutely nothing in science or in scientific method to warrant us in assigning any limit to this hierarchy of laws. When in many undoubted cases we find law overriding law, and at certain points in our experience producing unexpected results, we can never venture to affirm that we have exhausted the strange phenomena which may have been provided for in the original constitution of matter. The Universe might have been so designed that it should for long intervals go through the same round of almost unvaried existence, and yet so that events of exceptional character should from time to time be produced. Charles Babbage showed in that most profound and eloquent work, 'The Ninth Bridgwater Treatise,' that it was theoretically possible for human artists to design a machine, consisting of metallic wheels and levers, which should work invariably by one simple law of action during any finite number of steps, and yet at a fixed moment, however distant, should manifest a single breach of law. Such an engine might go on counting, for instance, the natural numbers until they might reach a number requiring for its expression a hundred million digits. 'If every letter in the volume now before the reader's eyes,' says Babbage[e], 'were changed into a figure, and if all the figures contained in a thousand such volumes were arranged in order, the whole together would yet fall far short of the vast induction the observer would have had in favour of the truth of the law of natural numbers . . . Yet shall the engine, true to the prediction of its director, after the lapse of myriads of ages, fulfil its task, and give that one, the first and only exception to that time-sanctioned law. What would have been the chances against the appear-

[e] 'Ninth Bridgwater Treatise,' p. 140.

ance of the excepted case, immediately prior to its occurrence?'

As Babbage further showed[f], a calculating engine, after proceeding through any required number of motions according to a first law, may be made suddenly to suffer a change, so that it shall then commence to calculate according to a wholly new law. After giving the natural numbers for any finite time, it might suddenly begin to give triangular, or square, or cube numbers, and these changes might theoretically be conceived as occurring time after time. Now if such occurrences can be designed and foreseen by a human artist, it is surely within the capacity of the Divine Artist to provide for similar changes of law in the mechanism of the atom, or the construction of the heavens.

Physical science, so far as its highest speculations can be trusted, gives some indication of a change of law in the past history of the Universe. According to Sir W. Thomson's deductions from Fourier's Theory of Heat, we can trace down the dissipation of heat by conduction and radiation to an infinitely distant time when all things will be uniformly cold. But we cannot similarly trace the heat-history of the Universe to an infinite distance in the past. For a certain negative value of the time the formulæ give impossible values, indicating that there was some initial distribution of heat which could not have resulted, according to known laws of nature, from any previous distribution[g]. There are other cases in which a consideration of the dissipation of energy leads to the conception of a limit to the antiquity of the present order of things[h]. Human science, of course, is fallible, and

[f] 'Ninth Bridgwater Treatise,' pp. 34–43.

[g] Tait's 'Thermodynamics,' p. 38. 'Cambridge Mathematical Journal,' vol. iii. p. 174.

[h] Clerk Maxwell's 'Theory of Heat,' p. 245.

some oversight or erroneous simplification in these theoretical calculations may afterwards be discovered; but as the present state of scientific knowledge is the only ground on which erroneous interpretations of the uniformity of nature and the reign of law are founded, I am right in appealing to the present state of science in opposition to these interpretations. Now the theory of heat places us in the dilemma either of believing in Creation at an assignable date in the past, or else of supposing that some inexplicable change in the working of natural laws then took place. Physical science gives no countenance to the notion of infinite duration of matter in one continuous course of existence. And if in time past there has been a discontinuity of law, why may there not be a similar event awaiting the world in the future. Infinite ingenuity could have implanted some agency in matter so that it might never yet have made its tremendous powers manifest. We have a very good theory of the conservation of energy, but the foremost physicists do not deny that there may possibly be forms of energy, neither kinetic nor potential, and therefore of unknown nature[i].

We can imagine reasoning creatures dwelling in a world where the atmosphere was a mixture of oxygen and inflammable gas like the fire-damp of coal mines. If devoid of fire, they might have lived on through long ages in complete unconsciousness of the tremendous forces which a single spark could call into play. In the twinkling of an eye new laws might have come into action, and the poor reasoning creatures who were so confident in their knowledge of the uniform conditions of their world, might have had no time even to speculate upon the overthrow of all their theories. Can we with our finite knowledge be sure that such an overthrow of our theories is impossible?

i Maxwell's 'Theory of Heat,' p. 92.

The Ambiguous Expression.—Uniformity of Nature.

I have asserted that a serious misconception arises from an ambiguous interpretation of the expression Uniformity of Nature. Every law of nature is the statement of a certain uniformity observed to exist among phenomena, and since the laws of nature are supposed to be invariably obeyed it seems to follow that the course of nature itself is uniform, so that we can safely judge of the future by the present. This inference is supported by some of the most profound results of physical astronomy. Laplace proved that the planetary system was stable, so that no one of the perturbations which planet produces upon planet shall become so great as to cause a disruption, and a permanent alteration in the planetary orbits. A full comprehension of the law of gravity shows that all such disturbances are essentially periodic, so that after the lapse of millions of years the planets will all return to the same relative positions and a new cycle of disturbances will commence.

As other branches of inquiry progress, we seem to gain assurance that no great alteration of the world's condition is to be expected. A conflict with a comet has long been a cause of fear to some persons, but now it is credibly asserted that we have passed through a comet's tail without the fact being known at the time, or manifested by any more serious a phenomenon than a slight luminosity of the heavens. More recently still the earth is said to have actually touched the comet Biela, and the only result was a beautiful and perfectly harmless display of radiating meteors. A decrease in the heating power of the sun seems to be the next most probable circumstance from which we might fear an extinction of life on the earth. But calculations founded on reasonable physical

data show that no appreciable change can be going on, and experimental data to indicate any change are wholly wanting. Geological investigations show indeed that there have been extensive variations of climate in past times; vast glaciers and icebergs have swept over the temperate regions at one time, and tropical vegetation has flourished near the poles at another time. But here again the vicissitudes of climate assume a periodic character, so that the ultimate stability of the earth's condition does not seem to be affected.

All these statements may be reasonable, but they do not in the least establish the Uniformity of Nature in the sense that extensive alterations or sudden catastrophes are impossible. In the first place Laplace's theory of the stability of the planetary system is of an abstract character, as paying regard to nothing but the mutual gravitation of the planetary bodies and the sun. It overlooks several physical causes of change and decay in the system which were not so well known in his day as at present, and it also presupposes the absence of any interruption of the course of things by conflict with foreign astronomical bodies.

It is now commonly acknowledged by astronomers that there are at least two ways in which the *vis viva* of the planets and satellites may suffer loss. The friction of the tides upon the earth produces a small amount of heat which is radiated into space, and this loss of energy must result in a decrease of the rotational velocity, so that ultimately the terrestrial day will become identical with the year, just as the periods of revolution of the moon upon its own axis and around the earth have already become equal. Secondly, there can now be little doubt that various manifestations of electricity upon the earth's surface depend upon the relative motions of the planets and the sun, which give rise to various periods of increased intensity. Such electrical phenomena must result

in the production and dissipation of heat, the energy of which must be drawn, partially at least, from that of the moving bodies. This effect is probably identical, as I have suggested (vol. ii. p. 213), with the very evident loss of energy of comets attributed to a so-called resisting medium. But whatever be the theoretical explanation of these phenomena, it is almost certain that there exists a tendency to the dissipation of the energy of the planetary system, which will in the indefinite course of time result in the fall of the planets into the sun.

It is hardly probable, however, that the planetary system will be left undisturbed throughout the enormous period of time required for the dissipation of its energy in this way. Conflict with other bodies is so far from being improbable, that it becomes approximately certain when we take very long intervals of time into account. As regards cometary conflicts, I am by no means satisfied with the negative conclusions drawn from the remarkable display on the evening of the 27th of November, 1872. We may often have passed through the tails of comets, which are probably electrical manifestations no more substantial than the aurora borealis. Every remarkable shower of shooting stars may also be considered as proceeding from a cometary body, so that we may be said to have passed through the thinner parts of various comets. But the earth has probably never passed, in times of which we have any record, through the nucleus of a comet, which consists perhaps of a dense swarm of small meteorites. We can only speculate upon the effects which might be produced by such a conflict, but it would probably be a much more serious event than any yet registered in history. The probability of its occurrence, too, can hardly be assigned; for though the probability of conflict with any one cometary nucleus is almost infinitesimal, yet the number of comets is immensely great (vol. ii. p. 11).

It is far from impossible, again, that the planetary system may be invaded by bodies of greater mass than any comets. The sun seems to be placed in so extensive a portion of empty space, that its own proper motion would not bring it to the nearest known star (a Centauri) in less than 139,200 years. But in order to be sure that this long interval of undisturbed life is granted to our globe, we must prove that there are no stars moving so as to meet us, and no dark bodies of considerable size flying through intervening space unknown to us. The intrusion of comets into our system, and the fact that many of them have hyperbolic paths, is sufficient to show that the surrounding parts of space are occupied by multitudes of dark bodies of some size. It is quite probable that small suns might have cooled sufficiently to become non-luminous; for even if we discredit the theory that the variation of brightness of periodic stars is due to the revolution of dark companion stars, yet there is our own globe as an unquestionable example of a smaller body which has cooled below the luminous point.

Altogether, then, it is a mere assumption that the Uniformity of Nature involves the unaltered existence of our own globe. There is no kind of catastrophe which is too great or too sudden to be theoretically consistent with the reign of law. For all that our science can tell, human history may be closed in the next instant of time. The world may be dashed to pieces against some intruding body; it may be involved in a nebulous atmosphere of hydrogen to be exploded a second afterwards; it may be scorched up or dissipated into vapour by some great explosion in the sun; there might even be within the globe itself some secret cause of disruption, which only needs time for its manifestation.

There are even some indications, as already noticed (vol. ii. p. 327), that some violent disturbances have

actually occurred in the history of the solar system. Olbers sought for the minor planets or asteroids, on the supposition that they were fragments of an exploded or fractured planet, and he was rewarded with the discovery of some of them. The retrograde motion of the satellites of the more distant planets, the abnormal position of the poles of Uranus and the excessive distance of Neptune, are other indications of some violent event, of which we have no other evidence. I adduce all these facts and arguments, not to show that there is any appreciable probability, so far as we can judge, of actual interruption within the scope of human history, but to prove that the Uniformity of Nature is theoretically consistent with the most unexpected events of which we can form any conception.

Possible States of the Universe.

When we give the rein to scientific imagination, it becomes apparent that conflict of body with body must not be regarded as the rare exception, but as the general rule and the inevitable fate of each star system. So far as we can trace out the results of the law of gravitation, and the dissipation of energy, the universe must be regarded as undergoing gradual condensation into a single cold solid body of gigantic dimensions. Those who so frequently use the expression Uniformity of Nature, seem to forget that the universe might exist consistently with the laws of nature in the most diverse conditions. It might consist, on the one hand, of a glowing nebulous mass of gaseous substances. The heat might be so intense that all elements, even carbon and silicon, would resemble permanent gases, and all atoms, of whatever nature, would be flying about in chemical independence, diffusing themselves almost uniformly in the neighbouring parts of space. There would then be no life, unless we can

apply that name to the passage through each part of space of similar average trains of atoms, the particular successions of atoms being governed only by the theory of probability, and the law of divergence from a mean exhibited in the Arithmetical Triangle. Such a universe would correspond partially to the Lucretian rain of atoms, and to that nebular hypothesis out of which Laplace proposed philosophically to explain the evolution of the planetary system.

According to another extreme supposition, the intense heat energy of this nebulous mass might have been mostly radiated away into the unknown regions of outer space. The attraction of gravity would then have shown itself between each two particles, and the energy of motion thence arising would, by incessant conflicts, be resolved into heat and dissipated.

Inconceivable ages might be required for the completion of this process, but the dissipation of energy thus proceeding could end only in the production of a cold and motionless stone-like universe. The relation of cause and effect, as we see it manifested in life and growth, would then degenerate into the constant existence of every particle in a fixed position relative to every other particle. Logical and geometrical resemblances would still exist between atoms, and between groups of atoms crystallized in their appropriate forms for ever more. But time, the great variable, would bring no variation, and as to human hopes and troubles, they would have come to eternal rest.

Science is not really adequate to proving that such is the inevitable fate of the universe, for we can seldom trust our best established theories and most careful inferences far from their data. Nevertheless, the most probable speculations which we can form as to the history, especially of our own planetary system, is that it origi-

nated in a heated revolving nebulous mass of gas, and is in a state of almost infinitely slow progress towards the cold and stony condition. Other speculative hypotheses might doubtless be entertained. Every hypothesis is pressed by difficulties. If the whole universe be cooling, where does the heat go to? If we are to get rid of it entirely, outer space must be infinite in extent, so that it shall never be stopped and reflected back. But not to speak of metaphysical difficulties, if the medium of heat undulations be infinite in extent, why should not the material bodies placed in it be infinite also in number and quantity. It is quite apparent that we are venturing into speculations which altogether surpass our powers of scientific inference. But then I am arguing negatively; I wish only to show that those who speak of the uniformity of nature, and the reign of law, often misinterpret entirely the meaning involved in those expressions. Law is not inconsistent with extreme diversity, and, so far as we can read the history of this planetary system, it did most probably originate in heated nebulous matter, and man's history forms but a moment in its progress towards the cold and stony condition. It is by very doubtful and speculative hypotheses alone that we can avoid such a conclusion, and I depart least from undoubted facts and well-established laws, when I assert that, whatever uniformities may underlie the phenomena of nature, constant variety and ever-progressing change is the real outcome.

Speculations on the Reconcentration of Energy.

There are unequivocal indications, as I have said, that the material universe, as we at present see it, is progressing from some act of creation, or some discontinuity of existence of which the date may be approxi-

mately fixed by scientific inference. It is progressing towards a state in which the available energy of matter will be dissipated through infinite surrounding space, and all matter will become cold and lifeless. This constitutes, as it were, the historical period of physical science, that over which our scientific insight may more or less extend. But in this, as in other cases, we have no right to interpret our experience negatively, so as to infer that because the present state of things began at a particular time, there was no previous existence. It may be that the present period of material existence is but one of an indefinite series of like periods. All that we can see, and feel, and infer, and reason about may be, as it were, but a part of one single pulsation in the existence of the universe.

After Sir W. Thomson had pointed out the preponderating tendency which now seems to exist towards the conversion of all energy into heat-energy, and its equal diffusion by radiation throughout space, the late Professor Rankine put forth a remarkable speculation[k]. He suggested that the ethereal, or rather, as I have called it, the *adamantine* medium in which all the stars exist, and all radiation takes place, may have bounds, beyond which only empty space may exist. All heat undulations reaching this boundary will be totally reflected, according to the theory of undulations, and will in all probability be reconcentrated into foci situated in many parts of the medium. Whenever a cold and extinct star happens to pass through one of these foci, it will be instantly ignited and resolved by intense heat into its constituent elements. A discontinuity will occur in the history of that portion of matter, and the star will begin its history afresh with a renewed store of energy.

This is doubtless a mere speculation, incapable of veri-

[k] 'Report of the British Association' (1852), Report of Sections, p. 12.

fication by observation, and almost free from any restrictions afforded by present knowledge. We might attribute various shapes to the whole body of adamantine medium, and the consequences would be various. But there is this value in such speculations, that they draw attention to the finiteness of our knowledge. We cannot deny the possible truth of such an hypothesis, nor can we place a limit to the scientific imagination in the framing of other like hypotheses. It is impossible, indeed, to follow out our scientific inferences without falling into speculation. If heat be radiated into outward space it must either proceed *ad infinitum*, or it must be stopped somewhere. In the latter case we fall upon Rankine's hypothesis. But if the material universe consist of a finite collection of heated matter situated in a finite portion of an infinite adamantine medium, then either this universe must have existed for a finite time, or else it must have cooled down during the infinity of past time indefinitely near to the absolute zero of temperature. I objected to Lucretius' argument against the destructibility of matter, that we have no knowledge whatever of the laws according to which it would undergo destruction. But we do know the laws according to which the dissipation of heat appears to proceed, and the conclusion inevitably is that a finite heated material body placed in a perfectly cold infinitely extended medium would in an infinite time become infinitely approximated to zero. Now our own world is not yet cooled down near to zero, so that physical science seems to place us in the dilemma of admitting either the finiteness of past duration of the world, or else the finiteness of the portion of medium in which we exist. In either case we become involved in metaphysical and mechanical difficulties surpassing our mental powers.

The Divergent Scope for New Discovery.

In the writings of some recent philosophers, especially of Auguste Comte, and in some degree John Stuart Mill, there is an erroneous and hurtful tendency to represent our knowledge as assuming an approximately complete character. At least these and many other writers fail to impress upon their readers a truth which I think cannot be too constantly borne in mind, namely, that the utmost successes which our scientific method can accomplish will not enable us to comprehend more than an infinitesimal fraction of what there doubtless is to comprehend. Professor Tyndall seems to me open to the same charge in a less degree. He remarks[1] that we can probably never bring natural phenomena completely under mathematical laws, because the approach of our sciences towards completeness may perhaps be asymptotic, so that however far we may go, there may still remain some facts not subject to scientific explanation. He thus likens the supply of novel phenomena to a convergent series, the earlier and larger terms of which have been successfully disposed of, so that only comparatively minor groups of phenomena remain for future investigators to occupy themselves upon. On the contrary, as it appears to me, the supply of new and unexplained facts is divergent in extent, so that the more we have explained, the more there is to explain. The further we advance in any generalization, the more numerous and intricate are the exceptional cases still demanding further treatment. The experiments of Boyle, Mariotte, Dalton, Gay-Lussac, and others, upon the physical properties of gases might seem to have exhausted that subject by showing that all gases obey the

[1] 'Fragments of Science,' p. 362.

same laws as regards temperature, pressure, and volume. But in reality these laws are only approximately true, and the divergences have afforded a wide and yet quite unexhausted field for further generalization. The more recent discoveries of Cagniard de la Tour and Professor Andrews might seem to have summed up many of these exceptional facts under a wider generalization, but in reality they have opened to us vast new regions of interesting inquiry, and they leave wholly untouched the question why one gas or one substance behaves differently from another.

The science of Crystallography is that perhaps in which the most precise and general laws have been detected, but it would be utterly untrue to assert that it has lessened the area of future discovery. We can show that each one of the seven or eight hundred forms of calcite is derivable by plain geometrical modifications from an hexagonal prism, but who has attempted to explain the molecular forces producing these modifications, or the chemical conditions in which they arise? The law of isomorphism is an important generalization, for it establishes a general resemblance between the forms of crystallization of natural classes of elements. But if we examine a little more closely we find that these forms are only approximately alike, and the divergence peculiar to each substance is an unexplained exception.

By many similar illustrations it might be readily shown that in whatever direction we extend our investigations and successfully harmonize a few facts, the result is only to raise up a host of other unexplained facts. Can any scientific man venture to state that there is less opening now for new discoveries than there was three centuries ago? Is it not rather true that we have but to open a scientific book and read a page or two, and we shall in all probability come to some recorded phenomenon of which

no precise explanation can yet be given? In every such fact there is a possible opening for new discoveries, and it can only be the fault of the investigator's mind if he can look around him and find no scope for the exercise of his faculties.

The Infinite Incompleteness of the Mathematical Sciences.

There is one privilege which a certain amount of knowledge should confer; it is that of becoming aware of the indefinite weakness of our powers compared with the tasks which they might undertake if stronger. To the poor savage who cannot count twenty, the arithmetical accomplishments of the ordinary schoolboy are miraculously great in comparison. The schoolboy cannot comprehend the almost infinitely greater powers of the student, who has acquired facility with algebraic processes. The student can but look with feelings of surprise and reverence at the powers of a Newton or Laplace. But the question at once suggests itself, Do the powers of the highest human intellect bear any moderate ratio to the things which are to be understood and calculated? How many further steps must we take in the rise of mental ability and the extension of mathematical method before we begin to exhaust the knowable?

I am inclined to find fault with mathematical writers because they often exult in what they can accomplish, but omit to point out that what they do is but an indefinitely, nay an infinitely, small part of what might be done. They exhibit a general inclination, with few exceptions, not to do so much as mention the existence of problems of an impracticable character. This may be excusable so far as the immediate practical result of their researches is in question, but the custom has the effect of misleading the

general public into the fallacious notion that mathematics is a *perfect* science, which accomplishes what it undertakes in a complete manner. On the contrary, it may be said that if a mathematical problem were selected by pure chance out of the whole variety which might be proposed, the probability is infinitely slight that a human mathematician could solve it. Just as the numbers we can count or frame to the mind are literally nothing compared with the numbers which might exist, so the whole accomplishments of a Laplace or a Lagrange are, as it were, the little corner of the multiplication table, which has really an indefinite extent.

I have sufficiently pointed out that the rude character of all our observations prevents us from being aware of the existence of the greater number of effects and actions of nature. It must be added that, if we perceived them, we should usually be incapable of including them in our theories from want of mathematical power. Some persons may be surprised that though nearly two centuries have elapsed since the time of Newton's discoveries, we have yet no general theory of molecular action. Some approximations have been made towards such a theory. Joule and Clausius have measured the velocity of gaseous atoms, or even determined the distance between the collision of atom and atom. Sir W. Thomson has approximated to the number of atoms in a given bulk of substance. Rankine has formed some reasonable hypotheses as to the actual constitution of atoms, but it would be a mistake to suppose that these ingenious results of theory and experiment form any appreciable approach to a complete solution of molecular motions. There is every reason to believe, judging from the spectra of the elements, and from other reasons, that even chemical atoms are very complicated structures. An atom of pure iron is probably a vastly more complicated system than that of the planets

and their satellites. A compound atom may perhaps be compared with a stellar system, each star a minor system in itself. The smallest particle of solid substance will consist of a vast number of such stellar systems united in regular order, each bounded by the other, communicating with it in some manner yet wholly incomprehensible. Now what are our mathematical powers in comparison with this problem?

After two centuries of continuous labour, the most gifted men have succeeded in calculating the mutual effects of three bodies each upon the other, under the simple hypothesis of the law of gravity. Concerning these calculations we must further remember that they are purely approximate, and that the methods would not apply where four or more bodies are acting, and all produce considerable effects each upon the other. There is every reason to believe that each constituent of a chemical atom must go through an orbit in the millionth part of the twinkling of an eye, in which it successively or simultaneously is under the influence of many other constituents, or possibly comes into collision with them. It is, I apprehend, no exaggeration to say that mathematicians have scarcely a notion of the way in which they could successfully attack so difficult a problem of forces and motions. Each of these particles is for ever solving differential equations, which, if written out in full, might perhaps belt the earth, as Sir J. Herschel has beautifully remarked[m].

Some of the most extensive calculations ever made, were those required for the reduction of the measurements executed in the course of the Trigonometrical Survey of Great Britain. The calculations arising out of the principal triangulation alone occupied twenty calculators during three or four years, in the course of which the

[m] 'Familiar Lectures on Scientific Subjects,' p. 458.

computers had to solve simultaneous equations involving seventy-seven unknown quantities. The reduction of the levellings again required the solution of a system of ninety-one equations. But these vast calculations present no approach whatever to what would be requisite for the complete treatment of any one physical problem. The motion of glaciers is supposed to be moderately well understood in the present day. A glacier is a viscid, slowly yielding mass, neither absolutely solid nor absolutely rigid, but it is expressly remarked by Forbes[n], that not even an approximate solution of the mathematical conditions of such a moving mass can yet be possible. 'Every one knows,' he says, 'that such problems are beyond the compass of exact mathematics;' but though mathematicians may know this, they do not often enough impress that knowledge on other people.

The problems which are solved in our mathematical books consist of a small selection of those which happen from peculiar conditions to be practicable. But the very simplest problem in appearance will often give rise to impracticable calculations. Mr. Todhunter[o] seems to blame Condorcet, because in one of his memoirs he mentions a problem to solve which would require

$$n + n' + n'' + n''' \quad -2$$

successive integrations. Now if our mathematical sciences are to pretend to cope with the problems which await solution, we must be prepared to effect an unlimited number of successive integrations; yet at present, and almost beyond doubt for ever, the probability that even a single integration, taken haphazard, will be found to come within our powers is exceedingly small.

In some passages of that most remarkable work, the

n 'Philosophical Magazine,' 3rd Series, vol. xxvi. p. 406.

o 'History of the Theory of Probability,' p. 398.

'Ninth Bridgwater Treatise[p],' Mr. Babbage has pointed out that if we had power to follow and detect the minutest effects of any disturbance, each particle of existing matter must be a register of all that has happened. 'The track of every canoe—of every vessel that has yet disturbed the surface of the ocean, whether impelled by manual force or elemental power, remains for ever registered in the future movement of all succeeding particles which may occupy its place. The furrow which it left is, indeed, instantly filled up by the closing waters; but they draw after them other and larger portions of the surrounding element, and these again, once moved, communicate motion to others in endless succession.' We may even say that 'The air itself is one vast library, on whose pages are for ever written all that man has ever said or even whispered. There, in their mutable but unerring characters, mixed with the earliest, as well as the latest sighs of mortality, stand for ever recorded, vows unredeemed, promises unfulfilled, perpetuating in the united movements of each particle, the testimony of man's changeful will[q].'

When we read truthful reflections such as these, we may congratulate ourselves that we have been endowed with minds which, rightly employed, can form some estimate of their incapacity, to trace out and account for all that proceeds in the simpler actions of material nature. It ought to be added that, wonderful as is the extent of physical phenomena open to our investigation, intellectual phenomena are yet vastly more extensive. Of this I might present one satisfactory proof were space available by pointing out that the mathematical functions employed in the calculations of physical science, form an infinitely small fraction of the functions which may be

[p] 'Ninth Bridgwater Treatise,' p. 115.

[q] Ibid. p. 113.

invented. Common trigonometry, for instance, consists of a great series of useful formulas, all of which arise out of the simple fundamental relation of the sine and cosine expressed in the one equation

$$\sin^2 x + \cos^2 x = 1.$$

But this is not the only trigonometry which may exist; mathematicians also recognise the so-called hyperbolic trigonometry of which the fundamental equation is

$$\cos^2 x - \sin^2 x = 1.$$

De Morgan has pointed out that the symbols of ordinary algebra form but three of an interminable series of conceivable systems[8]. As the logarithmic operation is to addition or addition to multiplication, so is the latter to a higher operation, and so on without limit.

We may rely upon it that indefinite, and to us inconceivable, advances will be made by the human intellect, in the absence of any unforeseen catastrophe to the species or the globe. Almost within historical periods we can trace the rise of mathematical science from its simplest germs. We can prove our descent from ancestors who counted only on their fingers, but how almost infinitely is a Newton or a Laplace above those simple savages. Pythagoras is said to have sacrificed a hecatomb when he discovered the Forty-seventh Proposition of Euclid, and the occasion was worthy of the sacrifice. Archimedes was beside himself when he first perceived his beautiful mode of determining specific gravities. Yet these great discoveries are the simplest elements of our schoolboy-knowledge. Step by step we can trace upwards the acquirement of new mental powers. What could be more wonderful and unexpected than Napier's discovery of logarithms, a wholly new mode of calculation which has multiplied perhaps a hundred-fold the working powers of every computer, and indeed has rendered easy calculations which

[8] 'Trigonometry and Double Algebra,' chap. IX.

were before almost impracticable. Since the time of Newton and Leibnitz whole worlds of problems have been solved which before were hardly conceived as matters of inquiry. In our own day extended methods of mathematical reasoning, such as the system of quaternions, have been brought into existence. What intelligent man will doubt that the recondite speculations of a Cayley or a Sylvester may possibly lead to some new methods, at the simplicity and power of which a future age will wonder, and yet wonder more that to us they were so dark and difficult. May we not repeat the words of Seneca: 'Veniet tempus, quo ista quæ nunc latent, in lucem dies extrahat, et longioris ævi diligentia: ad inquisitionem tantorum ætas una non sufficit. Veniet tempus, quo posteri nostri tam aperta nos nescisse mirentur.'

The Reign of Law in Mental and Social Phenomena.

After we pass from the so-called physical sciences to those which attempt to investigate mental and social phenomena, the same general conclusions will hold true. No one will be found to deny that there are certain uniformities of thinking and acting which can be detected in reasoning beings, and so far as we detect such laws we successfully apply scientific method. But those who attempt thus to establish social or moral sciences, soon become aware that they are dealing with subjects of enormous perplexity. Take, for instance, the science of Political Economy. If a science at all, it must be a mathematical science, because it deals with quantities of commodities. But so soon as we attempt to draw out the equations expressing the laws of variation of demand and supply, we discover that they must have a complexity entirely surpassing our powers of mathematical treatment.

We may lay down the general form of the equations, expressing the demand and supply for two or three commodities among two or three trading bodies, but all the functions involved are of so complicated a character that there is not much fear of scientific method making a rapid progress in this direction. If such be the prospects of a comparatively formal science, like Political Economy, what shall we say of Moral Science? Any complete theory of morals must deal with quantities of pleasure and pain, as Bentham pointed out, and must sum up the general tendency of each kind of action upon the good of the community. If we are to apply scientific method to morals, we must have a calculus of moral effects, a kind of physical astronomy investigating the mutual perturbations of individuals. But as astronomers have not yet fully solved the problem of three gravitating bodies, when shall we have a solution of the problem of three moral bodies?

Now the sciences of political economy and morality are, comparatively, abstract and general, treating mankind from simple points of view, and attempting to detect general grounds of action. They are to social phenomena what the general sciences of chemistry, heat, and electricity, are to the concrete science of meteorology. Before we can investigate the actions of any aggregate of men, we must have fairly mastered all the more abstract sciences applying to them, somewhat in the way that we have acquired a fair comprehension of the simpler truths of chemistry and physics. But all our physical sciences do not enable us to predict the weather two days hence with any great probability, and the general problem of meteorology is almost unattempted as yet. What shall we say then of the general problem of social science, which shall enable us to predict the course of events in a nation?

There have indeed been several writers who have pro-

posed to lay the foundations of the science of history. The late Mr. Buckle undertook to write the 'History of Civilisation in England,' and showed how the character of a nation could be explained by the nature of the climate and the fertility of the soil. He omitted to explain the contrast between the ancient Greek nation and the present one; either there must have been an extraordinary revolution in the climate and the soil, or some more complex causes must be imagined to have come into operation. Auguste Comte detected some very fundamental and simple laws of development through which nations pass. There are always three phases of intellectual condition,—the theological, the metaphysical, and the positive; and applying this general law of progress to concrete cases, Comte was enabled to predict that in the hierarchy of European nations, Spain would necessarily hold the highest place. Such are the parodies of science offered to us by the so-called positive philosophers.

A science of history in the true sense of the term is an absurd notion. A nation is not a mere sum of individuals whom we can treat by the method of averages; it is an organic whole, held together by ties of infinite complexity. Each individual acts and re-acts upon his own smaller or greater circle of friends, and those who acquire a public position, exert an influence on much larger sections of the nation. There will always be a few great leaders of exceptional genius or opportunities, the unaccountable phases of whose opinions and inclinations sway the whole body, even when they are least aware of it. From time to time arise critical positions, battles, delicate negotiations, internal disturbances, in which the slightest incidents may profoundly change the course of history. A rainy day may hinder a forced march, and change the course of a campaign; a few in-

judicious words in a despatch may irritate the national pride; the accidental discharge of a gun may precipitate a collision, the effects of which will last for centuries. It is said that the history of Europe at one moment depended upon the question whether the look-out man upon Nelson's vessel would or would not descry a ship of Napoleon's expedition to Egypt which was passing not far off. In human affairs, then, the smallest effects may produce the greatest results, and in such circumstances the real application of scientific method is out of the question.

The Theory of Evolution.

Very profound philosophers have lately generalized concerning the production of living forms and the mental and moral phenomena regarded as their highest development. Mr. Herbert Spencer's Theory of Evolution purports to explain the origin of all specific differences, so that not even the rise of a Homer or a Beethoven would escape from his broad theories. The homogeneous is unstable and must differentiate itself, says Spencer, and hence comes the variety of human institutions and characters. In order that a living form shall continue to exist and propagate its kind, says Mr. Darwin, it must be suitable to its circumstances, and the most suitable forms will prevail over and extirpate those which are less suitable. From these fruitful ideas are developed theories of evolution and natural selection which go far towards accounting for the existence of immense numbers of living creatures—plants, and animals. Apparent adaptations of organs and limbs to useful purposes, which Paley and other theologicans regarded as distinct products of creative intelligence, are now seen to follow as natural

effects of a constantly acting tendency. Even man, according to these theories, is no distinct creation, but rather an extreme specimen of brain development. His nearest cousins are the apes, and his pedigree extends backwards until it joins that of the lowliest zoophytes.

The theories of Darwin and Spencer are doubtless not demonstrated; they are to some extent hypothetical, just as all the theories of physical science are to some extent hypothetical, and open to doubt. But I venture to look upon the theories of evolution and natural selection in their main features as two of the most probable hypotheses ever proposed, harmonizing and explaining as they do immense numbers of diverse facts. I question whether any scientific works which have appeared since the 'Principia' of Newton, are comparable in importance with those of Darwin and Spencer, revolutionizing as they do all our views of the origin of bodily, mental, moral, and social phenomena.

Granting all this, I cannot for a moment admit that the theory of evolution will alter our theological views. That theory embraces several laws or uniformities which are observed to be true in the production of living forms; but these laws do not determine the size and figure of living creatures, any more than the law of gravitation determines the magnitudes and distances of the planets. Suppose that Darwin is correct in saying that man is descended from the Ascidians: yet the precise form of the human body must have been influenced by an infinite train of circumstances affecting the reproduction, growth, and health of the whole chain of intermediate beings. No doubt, the circumstances being what they were, man could not be otherwise than he is, and if in any other part of the universe an exactly similar earth, furnished, with exactly similar germs of life, existed, a race must have grown up there exactly similar to the human race.

By a different distribution of atoms in the primæval world a different series of living forms on this earth must have been produced. From the same causes acting according to the same laws, the same results will follow; but from different causes acting according to the same laws, different results will follow. So far as we can see, then, infinitely diverse living creatures might have been created consistently with the theory of evolution, and the precise reason why we have a back-bone, two hands with opposable thumbs, an erect stature, a complex brain, about 223 bones, and many other peculiarities, is only to be found in the original act of creation. I do not, any less than Paley, believe that the eye of man manifests design. I believe that the eye was gradually developed, and we can in fact trace its gradual development from the first germ of a nerve affected by light rays in some simple zoophyte. In proportion as the eye became a more delicate and accurate instrument of vision, it enabled its possessor to escape destruction, but the ultimate result must have been contained in the aggregate of the causes, and these causes, so far as we can see, were subject to the arbitrary choice of the Creator.

Although Professor Agassiz is clearly wrong in holding that every species of animals or plants has appeared on earth by the immediate intervention of the Creator, which would amount to saying that no laws of connexion between forms are discoverable, yet he seems to be right in asserting that living forms are entirely distinct from those produced from purely physical causes. 'The products of what are commonly called physical agents,' he says[u], 'are everywhere the same (*i. e.* upon the whole surface of the earth) and have always been the same (*i. e.* during all geological periods); while organized beings are everywhere different and have differed in all ages. Between two such series

[u] Agassiz, 'Essay on Classification,' p. 75.

of phenomena there can be no causal or genetic connexion.' Living forms as we now regard them are essentially variable. Now from constant mechanical causes constant effects would ensue. If vegetable cells are formed on geometrical principles, being first spherical, and then by mutual compression dodecahedral, then all cells should have similar forms. In the Foraminifera and some other of the more lowly organisms, we do seem to observe the production of complex forms on pure geometrical principles. But from similar causes acting according to similar laws and principles, only similar results could be produced. If the original life-germ of each creature is a simple particle of protoplasm, unendowed with any distinctive forces, then the whole of the complex phenomena of animal and vegetable life are effects without causes. Protoplasm may be chemically the same substance, and the germ-cell of a man and of a fish may be apparently the same, so far as the microscope can decide; but if certain cells produce men and others as uniformly produce a given species of fish, there must be a hidden constitution determining the extremely different results. If this were not so, the generation of every living creature from the uniform germ would have to be regarded as a distinct act of arbitrary creation.

Theologians have dreaded the establishment of the theories of Darwin and Spencer, as if they thought that those theories could explain everything upon the purest mechanical and material principles, and exclude all notions of design. They do not see that those theories have opened up more questions than they have closed. The doctrine of evolution gives a complete explanation of no single living form. While showing the general principles which prevail in the variation of living creatures, it only points out the infinite complexity of the causes and circumstances which have led to the present state of things.

Any one of Mr. Darwin's books, admirable though they all are, consists but in the setting forth of a multitude of indeterminate problems. He proves in the most beautiful manner that each flower of an orchid is adapted to some insect which frequents and fertilizes it, and these adaptations are but a few cases of those immensely numerous ones which have occurred throughout the life of plants and animals. But why orchids should have been formed so differently from other plants, why anything, indeed, should be as it is, rather than in some of the other infinitely numerous possible modes of existence, he can never show. The origin of everything that exists is wrapped up in the past history of the universe. At some one or more points in past time there must have been arbitrary determinations which led to the production of things as they are.

Possibility of Divine Interference.

I will now draw the reader's attention to pages 168–171 of the first volume. I there pointed out that all inductive inference involves the assumption that our knowledge of what exists is complete, and that the conditions of things remain unaltered between the time of our experience and the time to which our inferences refer. Recurring to the illustration of a ballot-box, employed in the Chapter on the Inverse Method of Probabilities, we assume when predicting the probable nature of the next drawing, that our previous drawings have been sufficiently numerous to give us nearly complete knowledge of the contents of the box; and, secondly, that no interference with the ballot-box takes place between the previous and the next drawings. The results yielded by the theory of probabilities are quite plain. No finite number of casual drawings can give us sure knowledge of the contents of the box, so that,

even in the absence of all disturbance, our inferences are merely the best which can be made, and do not approach to infallibility. If, however, interference be possible, even the theory of probability ceases to be applicable, for, the amount and nature of that interference being arbitrary and unknown, there ceases to be any connexion between premises and conclusion. Many years of reflection have not enabled me to see any way of avoiding this hiatus of scientific certainty. The conclusions of scientific inference appear to be always of an hypothetical and purely provisional nature. Given certain experience the theory of probability yields us the true interpretation of that experience and is the surest guide open to us. But the best calculated results which it can give are never absolute probabilities; they are purely relative to the extent of our information. It seems to be impossible for us to judge how far our experience gives us adequate information of the universe as a whole, and of all the forces and phenomena which can have place therein.

I feel that I cannot in the space remaining at my command in the present volume, sufficiently follow out the lines of thought suggested, or define with precision my own conclusions. This chapter contains merely *Reflections* upon subjects of so weighty a character that I should myself wish for many years—nay for more than a lifetime of further reflection. My purpose, as I have repeatedly said, is the purely negative one of showing that atheism and materialism are no necessary results of Scientific Method. From the preceding reviews of the value of our scientific knowledge, I draw one distinct conclusion, that we cannot disprove the possibility of Divine interference in the course of nature. Such interference might arise, so far as our knowledge extends, in two ways. It might consist in the disclosure of the existence of some agent or spring of energy previously unknown, but which effects a

given purpose at a given moment. Like the pre-arranged change of law in Babbage's Imaginary Calculating Machine, there may exist pre-arranged surprises in the order of nature, as it presents itself to us. Secondly, the same Power, which created material nature, might, so far as I can see, create additions to it, or annihilate portions which do exist. Such events are doubtless inconceivable to us in a certain sense; yet they are no more inconceivable than the existence of the world as it is. The indestructibility of matter, and the conservation of energy, are very probable scientific hypotheses, which accord very satisfactorily with experiments of scientific men during a few years past, but it would be a gross misconception of scientific inference to suppose that they are certain in the sense that a proposition in geometry is certain, or that any fact of direct consciousness is certain in itself. Philosophers no doubt hold that *de nihilo nihil fit*, that is to say, their senses give them no means of imagining to the mind how creation can take place. But we are on the horns of a trilemma; we must either deny that anything exists, or we must allow that it was created out of nothing at some determinate date, or that it existed from past eternity. The first alternative is absurd; the other two seem to me equally conceivable.

Conclusion.

It may seem that there is one point where our speculations must end, namely, where contradiction begins. The laws of Identity and Difference and Duality were the very foundations from which we started, and they are, so far as I can see, the foundation which we can never quit. Scientific Method must begin and end with the laws of thought, but it does not follow that it will save us from encountering inexplicable, and at least apparently contra-

dictory results. The very nature of continuous quantity leads us into extreme difficulties. Any finite length is composed of an infinite number of infinitely small spaces, each of which, again, is composed of an infinite number of spaces of a second order of infinite smallness; these spaces of the second order are composed, again, of infinitely small spaces of the third order. Even these spaces of the third order are not absolute geometrical points answering to Euclid's definition of a point, as position without magnitude. Go on as far as we will, in the subdivision of continuous quantity, yet we never get down to the absolute point. Thus Scientific Method leads us to the inevitable conception of an infinite series of successive orders of infinitely small quantities. If so, there is nothing impossible in the existence of a myriad universes within the compass of a needle's point, each with its stellar systems, and its suns and planets, in number and variety unlimited. Science does nothing to reduce the number of strange things that we may believe. When fairly pursued it makes large drafts upon our powers of comprehension and belief.

Some of the most precise and beautiful theorems in mathematical science seem to me to involve apparent contradiction. Can we imagine that a point moving along a perfectly straight line towards the west, would ever get round to the east and come back again, having performed a circuit through infinite space, as it were, yet without ever diverging from a perfectly straight direction? Yet this is what happens to the intersecting point of two straight lines, when, being in the same plane, one line revolves about a fixed point. The same principle is exhibited in the hyperbola, which may be regarded as an infinite ellipse, one extremity of which has passed to an infinite distance and come back in the opposite direction. A varying quantity may change

its sign by passing, as mathematicians say, either through zero or through infinity. In the latter case there must be one intermediate value of the variable for which the variant is indifferently negative infinity and positive infinity. Mathematicians may shirk the difficulty, but they cannot make this common result of mathematical principles appear otherwise than contradictory to our common notions of space.

The hypothesis that there is a Creator at once all powerful and all benevolent is surrounded, as it must seem to every candid investigator, with difficulties verging closely upon logical contradiction. The existence of the smallest amount of pain and evil would seem to show that He is either not perfectly benevolent, or not all-powerful. No one can have lived long without experiencing sorrowful events of which the significance is inexplicable. But if we cannot succeed in avoiding contradiction in our notions of elementary geometry, can we expect that the ultimate purposes of existence shall present themselves to us with perfect clearness? I can see nothing to forbid the notion that in a higher state of intelligence much that is now obscure may become clear. We perpetually find ourselves in the position of finite minds attempting infinite problems, and can we be sure that where we see contradiction, an infinite intelligence might not discover perfect logical harmony?

From science, modestly pursued, with a due consciousness of the extreme finitude of our intellectual powers, there can arise only nobler and wider notions of the purpose of Creation. Our philosophy will be an affirmative one, not that false and negative one of Auguste Comte, which has usurped the name, and misrepresented the tendencies of a true positive philosophy. Our science will not deny the existence of things because they cannot be weighed and measured. It will rather lead us to believe

that the wonders and subtleties of possible existence surpass all that our mental powers allow us clearly to perceive. The study of abstract logical and mathematical forms has seemed to convince me that even space itself is no requisite condition of conceivable existence. Everything, we are told by materialists, must be here or there, nearer or further, before or after. I deny this—and point to logical relations as my proof.

There formerly seemed to me to be something highly mysterious in the denominators of the binomial expansion (vol. i. p. 216) which are reproduced in that strange natural constant *e*, or

$$1+\frac{1}{1}+\frac{1}{1\cdot 2}+\frac{1}{1\cdot 2\cdot 3}+\ldots\ldots$$

and in many results of mathematical analysis. I now perceive, as already partially explained (vol. i. pp. 40–42, 180, 181, 443, 444), that they arise out of the fact that the relations of space do not apply to the logical conditions which govern the numbers of combinations as contrasted to those of permutations. So far am I from accepting Kant's doctrine that space is a necessary form of thought, that I regard it as an accident, and an impediment to pure logical reasoning. Material existences must exist in space no doubt, but intellectual existences may be neither in space nor out of space; they may have no relation to space at all, just as space itself has no relation to time. For all that I can see, then, there may be intellectual existences to which both time and space are nullities.

Now among the most unquestionable rules of Scientific Method is that first law that whatever phenomenon is, is. We must ignore no existence whatever; we may variously interpret or explain its meaning and origin, but if a phenomenon does exist it demands some kind of explanation. If then there is to be a competition for scientific recog-

nition, the world without us must yield to the undoubted existence of the spirit within. Our own hopes and wishes and determinations are the most undoubted phenomena within the sphere of consciousness. If men do act, feel, and live as if they were not merely the brief products of a casual conjunction of atoms, but the instruments of a far-reaching purpose, are we to record all other phenomena and pass over these? We investigate the instincts of the ant and the bee and the beaver, and discover that they are led by an inscrutable agency to work towards a distant purpose. Let us be faithful to our scientific method, and investigate also those instincts of the human mind, by which man is led to work as if the approval of a Higher Being were the aim of life.

THE END.

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OXFORD:

BY E. B. GARDNER, E. PICKARD HALL, AND J. H. STACY,

PRINTERS TO THE UNIVERSITY.

00069469

DUE DATE

19 JUN 1986

Digitized with financial assistance from the
Government of Maharashtra
on 18 July, 2018